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THE WEBER-FECHNER LAW AND THE ALL-OR-NONE THEORY*

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I

It is probable that there has never been a quantitative generalization in the history of experimental psychology which has attracted as much attention and has provoked as many controversies as has the so-called Weber-Fechner Law. Weber's early experiments (28) on pressure and lifted weights may be regarded as the foundation stone upon which Fechner reared his system of psychophysics.

Weber's law states that "In comparing objects and observing the distinction between them, we perceive not the difference between the objects but the ratio of this difference to the magnitude of the objects compared" [Titchener (25)]. The law has been formulated as

$$\frac{\Delta R}{R} = \text{constant},$$

where R is the magnitude of the stimulus and ΔR is an increment to this magnitude such that $R + \Delta R$ can just be perceived as greater than R. The evidence upon which this generalization was founded was surprisingly meager. Weber's experiments on pressure and lifted weights were very casual [Titchener (25)]. Nevertheless in Fechner's hands the "law" was advanced as a possible universal relation between sensation and stimulus, and became a point of departure for subsequent violent controversies over quantitative measurements of sensory experience.

Fechner extended the scope of the generalization by the process of integrating the function $\Delta S = k \frac{\Delta R}{R}$ after assuming that the ratio $\frac{\Delta R}{R}$ represents a unitary value of sensation S. He obtained

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the relation $S = k \log R + C$, where S is regarded as measured on a scale of sensation. The validity of an integration of this type has often been called in question, since if sensory scales appear to be composed of discontinuous magnitudes the discrete "just noticeable differences" make integration illegitimate. Delboef [Titchener (25)], on the other hand, has pointed out that the sensory scale might be treated as continuous since any stimulus magnitude between any two upper and lower limens may be sensed and serve as the starting-point for a scale. This argument has never been very convincing to those who object to treating the sensory scale as continuous under the conditions governing the taking of the actual measurements, and the tendency has been to avoid the logical difficulty by regarding the formulation $S = k \log R$ as an empirical one essentially as respectable as the older expression $\frac{\Delta R}{R} = \text{constant}$.

It is not the purpose of this paper to reconsider historical controversies, nor to review the development of the Weber-Fechner Law. These matters are all readily available in detail elsewhere [cf. Titchener (25)]. At the present time the Weber-Fechner Law is regarded as perhaps the best known generalization of quantitative psychology. Psychologists are still concerned with its theoretical interpretation and with the extent to which it applies to data of various sense modalities. It has been found that the law seems to apply approximately to intermediate values of stimulus intensities, i.e., $\frac{\Delta R}{R}$ is nearly constant for a wide variety of sensory phenomena, but that for extremes of low and of high intensity the law is found not to hold. A variety of interpretations have been advanced to account for the "law" and are recorded in the textbooks. These interpretations for the most part are of a verbalistic character and none of them has received general acceptance.

The writer wishes to emphasize a point of view which seems to him to account for the Weber-Fechner relation in the various sense departments in terms of the all-or-none nature of nerve mechanics. The following preliminary statement may serve to clarify the subsequent discussion.

1) The so-called Weber-Fechner Law seems to be but a rough and purely empirical generalization of little theoretical significance.

2) The curve relating intermediate ranges of stimulus intensities to sensory reactions is probably seldom truly logarithmic. The deviations from the supposed logarithmic law at extremes of the stimulus range, generally regarded as a "break-down of the Weber-Fechner rule," are usually parts of a continuous function consistent with known features of the mechanics of the nervous system.

3) The curve relating the amount of excitation to the logarithm of the stimulus is as a rule not actually linear, but is an ogive or a

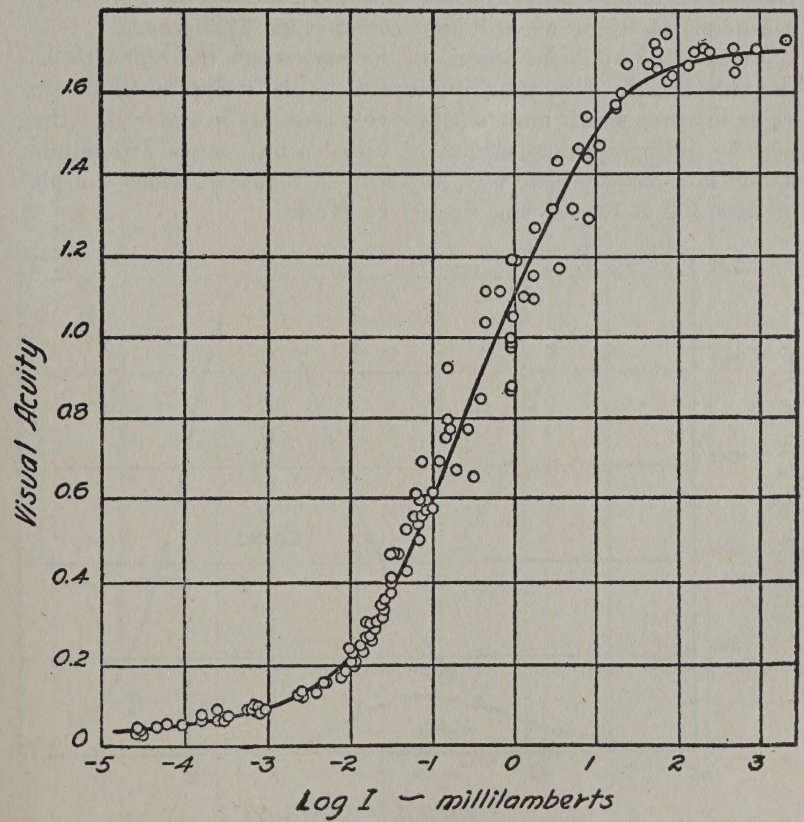

FIGURE 1

PLOT OF KOENIG'S DATA RELATING VISUAL ACUITY TO THE LOGARITHM OF THE LIGHT INTENSITY

(This figure is reproduced, with permission, from Hecht's paper in the *Journal of General Physiology.*)

sum of several ogive curves, representing integral distribution curves for thresholds of end-organs or afferent nerve-fibers.

II

In 1928 Hecht (16) published a paper on visual acuity which accounted for the Weber-Fechner Law in vision in terms of the position described in the foregoing paragraphs. Hecht had dealt with the application of the Weber-Fechner Law to photosensory phenomena in two earlier papers (14, 15), but for the purposes of a general treatment we will first consider the 1928 paper.

Visual acuity may be defined as the capacity of the retina to distinguish details. It is usually measured as the reciprocal of the angular distance which must separate two contours in order that they may be distinguished as separate. Visual acuity varies with illumination in a characteristic way, as shown in Figure 1, which is a plot of data due to Koenig and figured by Hecht.

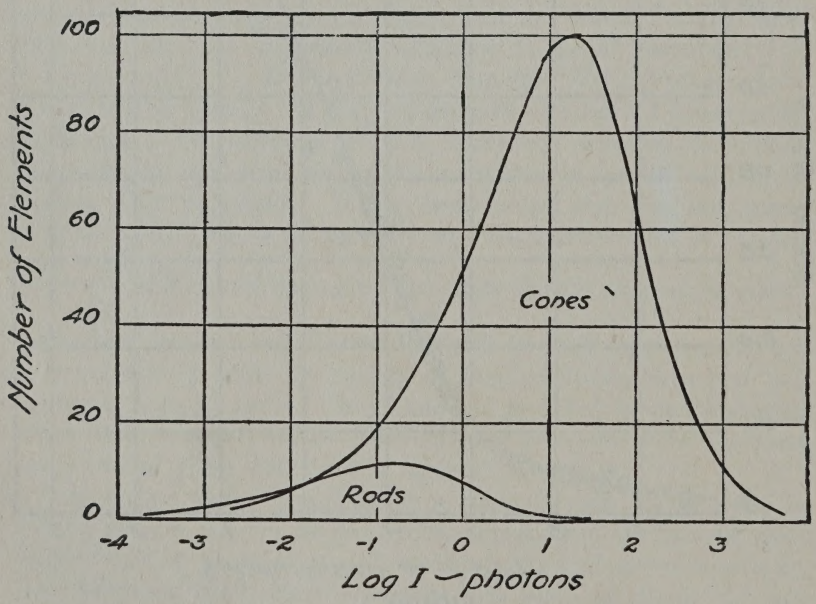

FIGURE 2

DISTRIBUTION CURVE FOR THE THRESHOLDS OF RODS AND CONES
(Reproduced, with permission, from Hecht's acuity paper in the *Journal of General Physiology*.)

The fineness of discriminable detail depends upon the number of receiving elements per unit area of the retina. But the number of rods and cones is anatomically fixed. Therefore, Hecht points out, some functionally determined change in the number of active rods

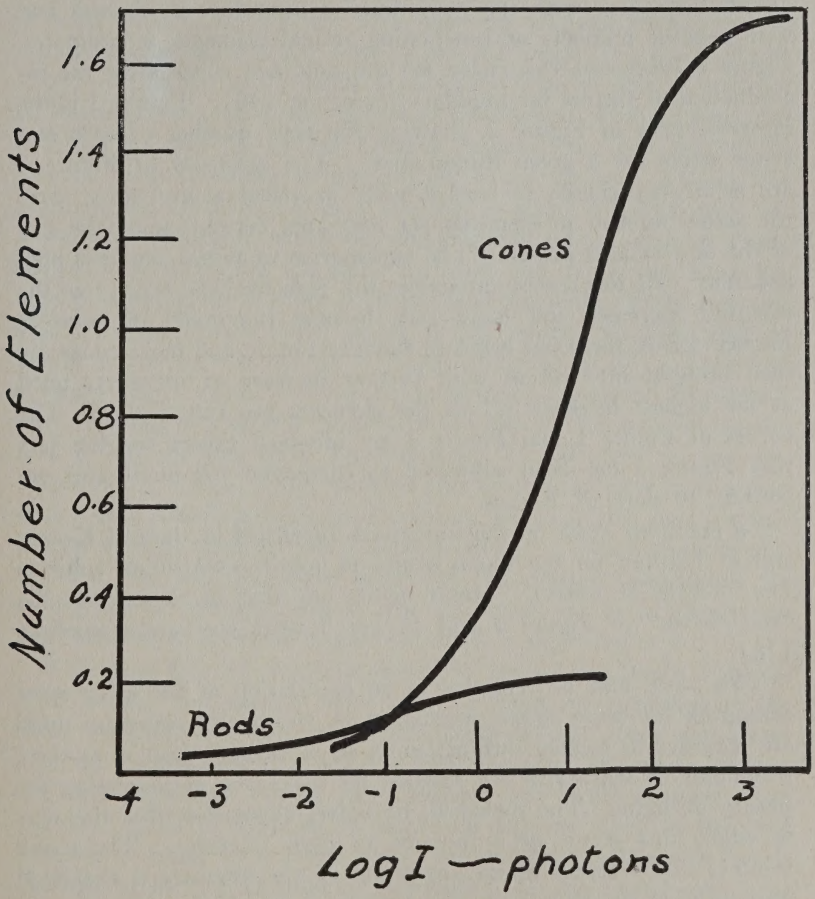

FIGURE 3

Integral curves of those shown in Figure 2 showing statistical sensibility of rods and cones. The ordinates are chosen arbitrarily to read in visual acuity units. These curves have been corrected for pupil size; their sum is otherwise identical with the curve of Figure 1. (This figure is copied, with permission, from Hecht's paper in *The Journal of General Physiology*.)

and cones must be postulated. In order to do this he assumes that sensibility of the individual rods and cones is not the same but is distributed in relation to the light intensity in the manner of populations, errors, and frequency distributions generally.

On the basis of this assumption Hecht calculated a frequency distribution curve with the logarithms of intensities as abscissa and with relative numbers of functioning retinal elements as ordinates. Figure 2 represents this curve for the rods and cones after the introduction of factors for pupillary correction (16). Figure 3 is the integral curve of Figure 2, showing the *total* number of rods and cones active for a given illumination. The ordinates of Figure 3 are arbitrarily chosen to read directly in visual acuity units, since the actual number of elements per unit area depends upon the size of the unit of area chosen. The explanation of visual acuity is now apparent. At the lowest intensities the rods mediate acuity; as the intensity increases and more rods become functional, a point is reached where the cones begin to function; more and more cones are then brought into action with further increase of intensity, until at the highest intensity all of the elements are functioning. The curves of Figure 1 and Figure 3 are identical except for the fact that Figure 3 has been subjected to correction for pupil area according to data of Reeves.

An excellent check on this analysis is furnished by data of Koenig and of Uhthoff on the visual acuity of totally color-blind subjects (i.e., devoid of cones). Hecht points out that in these cases the "rod curves" of Figure 2 and Figure 3 were accurately obtained (16).

This work may be regarded as an application of the all-or-none conception of sense organ mechanics to vision; the receptor units are regarded as coming into action in an all-or-none fashion as their thresholds of sensitivity are reached on the curve of increasing intensity of light. The *S*-shaped, or ogive, curve describes the way in which this occurs as a function of light intensity. The curve relating visual acuity to the logarithm of the intensity is therefore seen to be at no point truly logarithmic, as would be called for by the Weber-Fechner Law, but is only approximately so at its mid region.

Hecht's extension of the all-or-none law to vision is checked by a calculation of the actual number of cones in the minimal retinal

area that will just mediate the entire range of acuities. Hecht points out that there are 572 discrete discriminable brightness steps. Of these, 30 may be regarded as "rod steps," leaving 542 determined by the cones. If the recognition of a minimum change of intensity represents a change from n to $n + 1$ or to $n - 1$ in the number of cones functioning per unit area, then the minimal retinal area in the fovea must contain not fewer than 542 cones. To quote Hecht (16): "The minimal retinal area should also be able to mediate all visual acuities, from the highest to the lowest. Such an area will have for its side the retinal distance between two just perceptible contours corresponding to the lowest visual acuity. The retinal distances for all other values of the visual acuity will obviously fall within this distance. Moreover, this minimal retinal area for visual acuity is by definition the same for rods and for cones, since the unit of visual acuity is taken to be the same for both. From Koenig's data . . . the lowest visual acuity is 0.03 units. Koenig's unit of visual acuity corresponds to a visual angle of 1′20″. Therefore a visual acuity of 0.03 refers to an angular separation of slightly over 44′, and to a retinal distance of 0.2 mm. The resulting square area is 0.04 sq. mm. The fovea contains 13,500 cones per sq. mm. (Helmholtz, 1896). The minimal area derived directly from visual acuity data therefore contains 540 cones, which is the same number as that derived from intensity discrimination."[2] The correspondence between 540 and 542 cones is indeed phenomenal considering the divergence of methods used in the two cases, but it is merely the agreement as to order of magnitude which is significant.

The same type of reasoning has been applied by Hecht (16) to color vision. He assumes three types of cones corresponding to the three substances of Helmholtz, and suggests that these cones may have their thresholds distributed as have the cones as a whole, so that the cone-population curves just discussed are really composites of the three types. Perception of brightness depends, then, on the total number of cones in a given area; and brightness-discrimination is determined by the addition or subtraction of one functioning cone in this area. In a like manner, color and hue depend upon the

---

[2]The unit of acuity corresponds to 1′20″ or 80″. Therefore, 1/80 is the unit of acuity and $1/80 \times .03 = .000375$. $\frac{1}{.000375} = 2670''$, or $\frac{2670}{60} = 44'$.

relative number of the three kinds of cones functioning in the unit area; and hue discrimination at constant brightness is determined by the addition or subtraction of one functional cone of a specific type. To quote Hecht (16): "We found that a minimal retinal area contains 540 cones. Granting three cone species of approximately equal numbers we get about 180 cones per species per unit area. If, now, a spectral hue discrimination involves *on the average* one of each species of cone, then the number of monochromatic patches obtainable with a spectrum is about 180. It is significant that Koenig (1894-95) obtained 160 such patches; Nutting (1920) between 130 and 180; Laurens and Hamilton (1923), 161 and 207 respectively."

As Hecht points out, this statistical treatment of the thresholds for sensitivity in the rods and cones is not an arbitrary one, but is consistent with what is known of the photochemical properties of the retina. Earlier studies of Hecht (13) on dark adaptation and other phenomena of photic excitation have shown that in the rods and cones there very likely exists a reversible photochemical system which may be written

$$S \underset{\text{Dark}}{\overset{\text{Light}}{\rightleftharpoons}} P + A$$

where S is a photosensitive substance and P and A are its two principal precursors and products of photolysis. The equation representing equilibrium for this system is

$$K\ I = \frac{X^2}{a - x}$$

where I is the intensity of light, x is the percentage of S changed to P and A at photic equilibrium, a is the initial concentration of S, and K is a constant (the equilibrium constant). From data published by a number of workers relating the just noticeable increment of intensity to the total intensity over a wide intensity-range, Hecht was able to find values of K for the rods and for the cones which would satisfy certain predetermined conditions concerning the constancy of $x_2 - x_1$, x_1 corresponding to the amount of x obtained by an illumination of intensity I and x_2 to that for an illumination of $I + \Delta I$. With values of $K = 100$ for rod vision and $K = 0.25$ for cone vision, Hecht (14, 15) calculated from previously published

experimental data on intensity discrimination the amount of x or of P and A present as a function of the logarithm of the intensity of illumination. The curve obtained is ogival, in fact it is *identical* with the integral distribution curve of Figure 3. The derivation of this curve from the equation of the stationary state follows logically if it is supposed that the retinal elements are uniformly distributed in relation to the concentration of x necessary to discharge an element. Hecht (16) points out that this amounts to making the total number of active elements a linear function of the concentration of x. The consequences of these findings for an understanding of the Weber-Fechner Law has been dealt with at length by Hecht in his paper of 1924.

The general conception here outlined does away with the notion of the Weber-Fechner Law as commonly understood ($S=k \log R+C$) for brightness discrimination in vision. The ogive curve [Hecht (15)] relating amounts of P and A to log I is at best only an approximation to a straight line in its mid region. It at no point is actually logarithmic. In earlier work Hecht (13) showed that the amount of P and A is a direct linear function of the amount of retinal excitation, so that it is safe to regard this formulation as an expression of that relation between stimulus and retinal response with which the Weber-Fechner formulation has been traditionally concerned.

III

Hecht's application of the notion of differential thresholds and of the all-or-none theory to account for the Weber-Fechner Law in vision suggests the possibility of the more general application of some such theory to cover the phenomena of other sense departments. A consideration of the work of Crozier and Pincus (10, 11) on geotropism provides data for the extension of the hypothesis. This work has been published in detail elsewhere so it need be summarized only briefly here.

Crozier and Pincus (10) showed that for the orientation of young rats on inclined planes, under suitably controlled conditions, one empirically obtains an approximately linear relation when θ, the angle of orientation of the animal on the plane, is plotted against the logarithm of the sin of α where α is the angle of slope of the plane. Since sin α is actually the gravitational component in the

plane, this is equivalent to saying that the angle θ is proportional to the logarithm of the intensity of the force causing orientation. If it can be shown that θ, empirically the mere description of a position, is also a measure of excitation, this would seem clearly to be an application of the Weber-Fechner Law. That θ is actually a measure of excitation is apparent from the following discussion which I shall quote from Crozier and Pincus (11).

"For a given θ there is equivalence of numbers of tension receptors, sense organs, activated on the two sides of the body, and constant frequency of excitation over gross intervals of time. Then θ is a direct measure of the total excitation experienced. It is on this

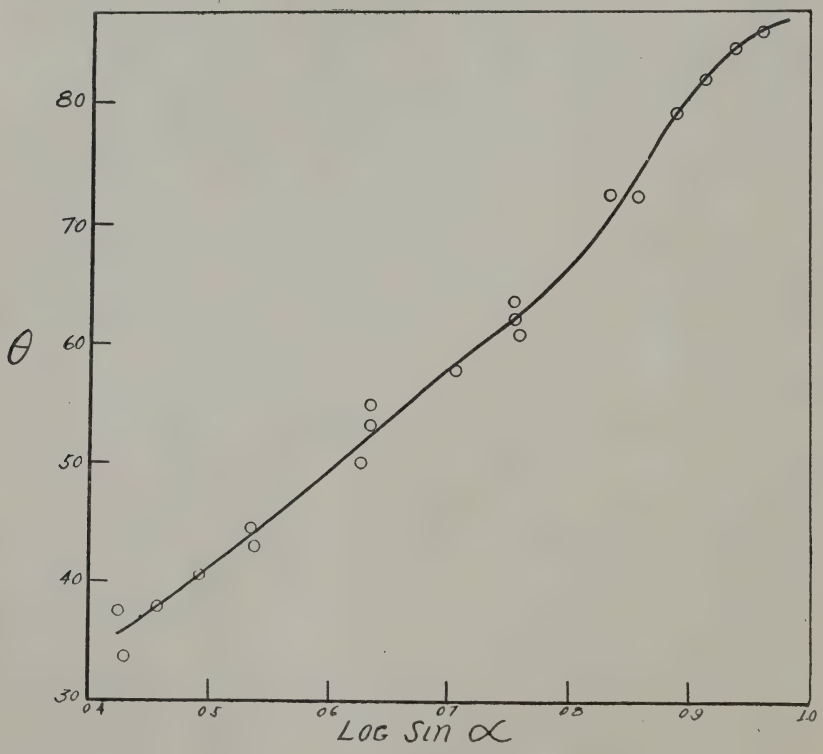

FIGURE 4

Curve from Crozier and Pincus showing pseudo-linear relation of Θ and log sin *a* for a strain of rats (the "K" race). The relation is actually a combination of ogive curves. (From *The Journal of General Physiology.*)

basis that the relation $\Delta \cos \theta / \Delta \sin \alpha = \text{const.}$[3] is obtained. When α is varied the speed of creeping changes, also θ. This signifies that both the total number of receptors and the frequency of excitation alter with α and so with θ. Hence θ is such a function of α that, as α varies, it in some fashion includes both elements in the totality of excitation over a gross unit of time. Assuming that for any small increase, ΔE, in total excitation per unit time, we have $\Delta E = K_1 (\Delta N) (\Delta F)$, where N signifies number of receptors, F frequency of excitation, due to changes in positions of limbs during progression; then since ΔF is proportional to the speed of progression, at least approximately, and since Δ speed $/\Delta \log \sin \alpha$ is sensibly constant, (Pincus, 1927), $\Delta E = K_2 (\Delta N) (\Delta \log \sin \alpha)$.

"But our assumption must be that θ and E are equivalent:

$$\Delta\theta = K_3 (\Delta N) (\Delta \log \sin \alpha),$$

and

$$\frac{\Delta\theta}{\Delta \log \sin \alpha} = K_3 \Delta N.$$

On this basis, plotting $\frac{\Delta\theta}{\Delta \log \sin \alpha}$ against $\sin \alpha$ should show how increasing α brings new sense organs into play according to the distribution of effective tension thresholds among them."[4]

It was found on close examination that the linear relation between θ and $\log \sin \alpha$ is not strictly maintained, but that the curve is in fact a long drawn-out ogive in all cases in which genetically pure stocks (i.e., comparable individuals) were examined (cf. Figure 4).

On plotting $\frac{\Delta\theta}{\Delta \log \sin \alpha}$ against the sin of α series of three differ-

[3]This relation (Crozier and Pincus, 1926) is a theoretical one derived from the assumption that the animal orients until the tension of the muscles of the legs on the two sides of the body is equal. The equation is derived from a consideration of the geometry of the animal and the gravitational field in which it is orienting. The experimental values of α and θ were found to fit it.

[4]Sin α is plotted as abscissa instead of log sin α since number of receptors activated depends directly upon the effective gravitational component, sin α.

ential population distribution curves became manifest (cf. Figure 5), representing three different groups of tension receptors.[5]

From the point of view of the Weber-Fechner Law it is interesting that here again the notion of differential sensitivity and all-or-none nervous system mechanics seems to account for the pseudo-logarithmic nature of the generalization. In this case, empirically,

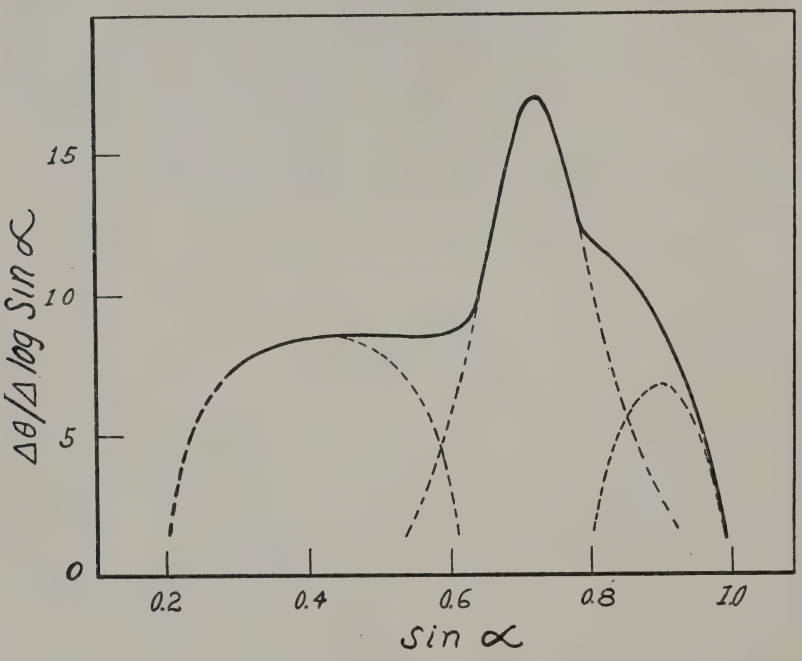

FIGURE 5

Curve, after Crozier and Pincus, showing relation $\frac{\Delta \theta}{\Delta \log \sin a}$ against sin a

This curve is the sum of three differential distribution curves, which are indicated by the broken lines. (After a figure in *The Journal of General Physiology*.)

[5]Crozier and Pincus have been able to show that these groups of receptors are inherited. Crossing of the genetically pure stock gives curves relating θ and log sin a predictable on a genetic basis. This work has introduced a functional conception of the gene, in that one deals with the inheritability of the "ability" of rats to respond to gravity over a wide range of values. The gene may be traced by the inheritability of a constant in a theoretically meaningful equation ultimately indicative, it would appear, of numbers of active *sense* organs.

the data give a combination of three integral population curves which became apparent on differentiation. With the geotropism data, in contrast to those for vision, the tendency at first was to regard the *whole* range of θ versus log sin α as perhaps linear and not only the mid region of the curve, as was done in the case of vision, the deviation from the linear in this latter instance being most extreme in the case for high and low values of intensity.[6]

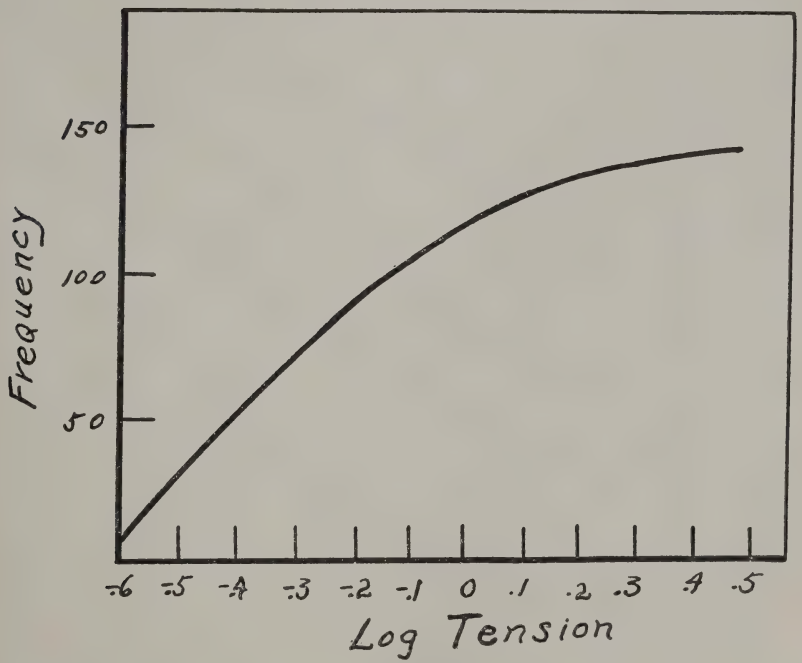

FIGURE 6

Curve showing the logarithm of the stimulus as a function of the frequency of responses from a single muscle spindle. This curve was made directly from the curve of Adrian and Zotterman (2) relating frequency to tension in the frog sterno-cutaneous preparation. The experimental points are not recorded, since in making this figure points were taking directly from the published curve.

[6]Excellent summaries of the work of both Hecht and Crozier are contained in the *Foundations of Experimental Psychology* (9, 17).

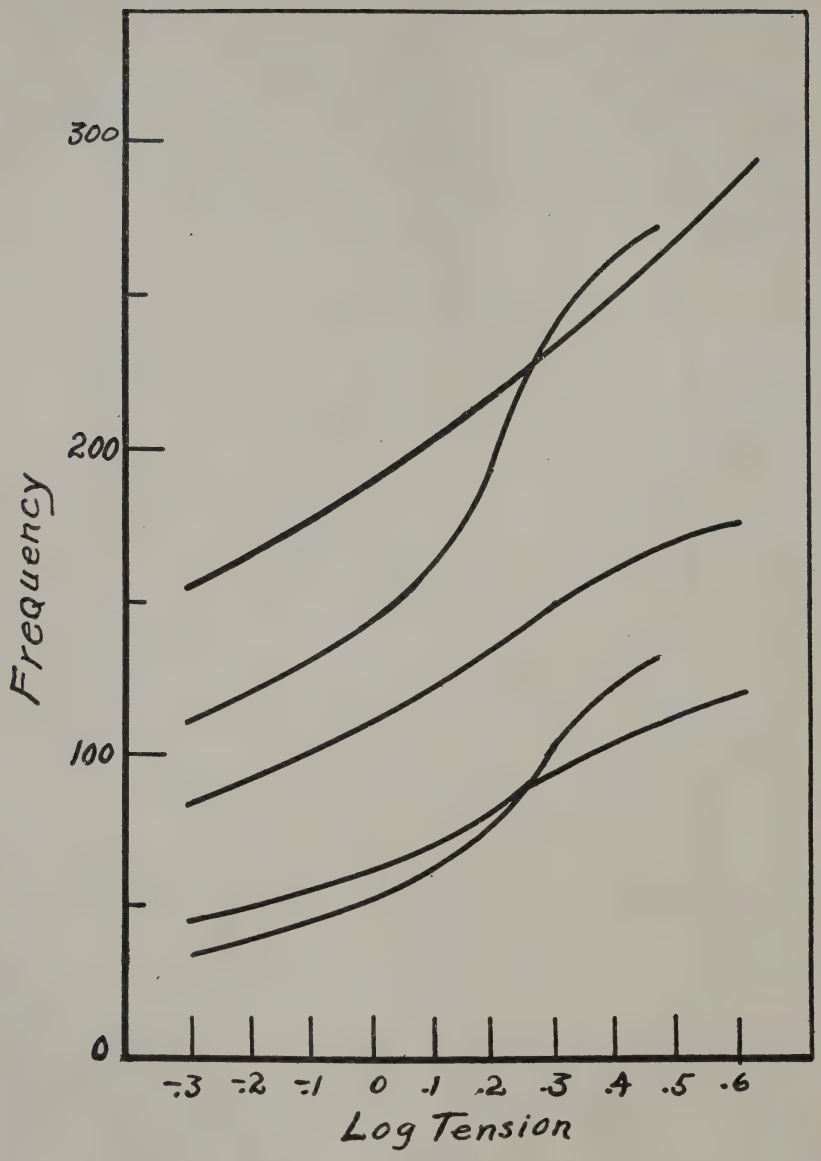

FIGURE 7

Curves made from data of five experiments by Adrian and Zotterman (2) relating frequency of afferent nerve discharge to the logarithm of the tension applied to certain intact frog muscles containing numerous end-organs. As in Figure 6, this curve was made from the published curves of Adrian and Zotterman relating frequency and tension. With one exception the ogive nature of the curves is apparent.

IV

The conception of the Weber-Fechner Law as a product of all-or-none nerve mechanics is further substantiated by recent work of Adrian (1). He has shown that the intensity of sensory reactions in the fields of vision, of muscle sense, and of pressure, is correlated with gross frequencies of impulse in the afferent nerves. This frequency is, of course, dependent upon two factors—the frequency of discharge of individual nerve-fibers, and the number of fibers active at any given time. This last factor is determined by the distribution of thresholds of the individual end-organs and nerve-fibers. Adrian and Zotterman (2) have analyzed the electrical response of a nerve connected with a single muscle spindle, in the sterno-cutaneous nerve-muscle preparation of the frog. Their curve relating tension on the muscle to frequency of afferent discharge is neither logarithmic nor ogival, as may be seen when the data are replotted as in Figure 6. If one assumes, however, a grading in thresholds of sensitivity for muscle receptors or for the nerve-fibers in general, it might well be that increased frequency of discharge would be due *primarily* to an increase in the number of functioning fibers despite the wide range of frequencies possible for a single receptor. It occurred to the writer that the data of Adrian and Zotterman for groups of end-organ cells might show the ogive form of curve if suitably plotted.

Figures 7 and 8 are plots made from data of Adrian and Zotterman in which they had plotted the gross frequency of afferent nerve responses directly against the stimulus for preparations containing many end-organs and nerve-fibers. Figure 7 [data of Adrian and Zotterman (2)] shows the logarithmic formulation of the relation between stimulus and frequency for myotatic reflexes of various intact frog muscles containing varying numbers of end-organs. The curves are distinctly ogival, as would be expected on the assumption of differential threshold distributions. Figure 8 is a replotting of data [Adrian and Zotterman (3)] relating pressure and frequency of afferent nerve discharge in the toe-pad of a cat. The three curves represent three different experiments. The nerve used supplies the toe-pad of the second digit and is a slender medial branch of the internal plantar, containing 200-400 fibers. The ogive nature of these curves is evident.

The data for Figure 7 were obtained after tension had been applied to the muscle for 10 seconds. Since frequency decreases with time, due to adaptation, and since the rate of decline may differ for different loads, one should, for a strict analysis, extrapolate the data in each case back to "zero" time. The amount of published data does not permit of this for the specific preparations recorded in Figures 7 and 8. Adrian and Zotterman (2) do, however, publish a figure (Figure 7, p. 162) which shows the decline in frequency of afferent impulses for a single muscle spindle with time for different tensions on the muscle. Examination of these curves shows that since the decline, a measure of the adaptation, is slightly

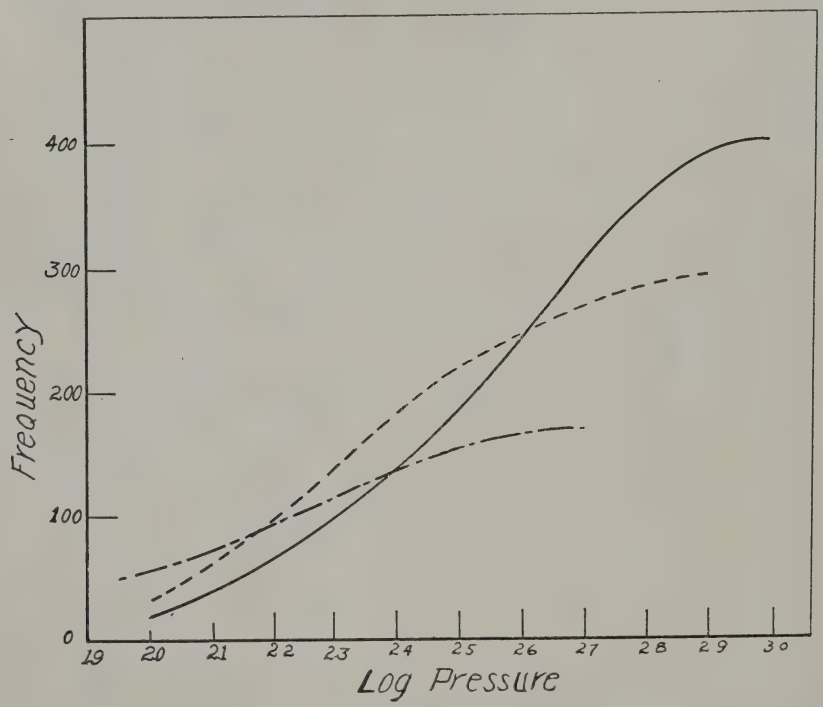

FIGURE 8

Curves made from data of three experiments of Adrian and Zotterman (3) relating frequency of afferent discharge to the logarithm of pressure applied to toe-pads of cats. The curves in this figure were made from points chosen from the published curves of the experimenters relating frequency and pressure. Their curves pass exactly through all of the experimental points. The ogive nature of the relation is apparent.

more rapid for greater loads, one would expect such an "adaptation correction" to increase the frequencies relatively more for the heavier than for the lighter loads recorded in Figure 7, thus tending slightly to straighten the curves for ranges of higher intensities. The frequencies of Figure 8 were obtained by averaging the frequencies for the first five seconds of stimulation [Adrian and Zotterman (3)]. In this case the adaptation rate is more nearly uniform for different intensities of pressure, as can be seen from Figure 9 of Adrian and Zotterman's paper. Owing to this fact and to the method of averaging frequencies for five-second intervals, one may assume that the adaptation correction would probably be negligible in this case.

At the present time there are not enough published data to separate the two types of frequency factors and of number of active units and frequencies of individual units, since the response of the single muscle spindle recorded in Figure 6 is for a different muscle than those used to obtain the data recorded in Figure 7. The ogival nature of the curves in Figure 7, however, can best be accounted for in terms of a conception of a distribution curve for differential thresholds. It is probable that while individual organs can, when forced, mediate a fairly wide range of frequencies, increasing the tension of a muscle containing many end-organs brings these organs into action according to their relative thresholds, thus accounting for the *S*-shape of the curves in Figure 7. That the differential conception may be correct in the case of the muscle sense is evident from the work of Crozier and Pincus just discussed. This conception has also been substantiated by work on the electrical responses of nerve-muscle preparations. The response of the single receptor (Figure 6) is in no sense ogival. The work of Adrian and Zotterman may therefore be regarded as substantiating the thesis concerning the nature of the Weber-Fechner Law. The responses of muscle receptors are consistent with the picture presented by the geotropic work of Crozier and Pincus, despite the great difference in approach to the problem on the part of Adrian and Zotterman. The results (cf. Figure 8) of the experiments with pressure applied to the cat toe-pad may be regarded as a further extension of the theory to the pressure sense.

V

While it seems probable that much of the data subsumed under

the Weber-Fechner Law can be accounted for in the manner described, the limitations of this type of explanation should be borne in mind. Many examples of relations between stimulus and response might be cited which are not even approximately either logarithmic or ogival on a semi-log plot. For example, the amount of response of earthworms to stimulation by acids is a *linear* function of the concentration of the acid [Crozier (5)]; reasons for this relation are pointed out in Crozier's paper. Patten (23) found that while under certain conditions of experimentation the Weber-Fechner relation seemed to hold between two light intensities and the orienting effect, with blow-fly larvae, that in general the law did not hold but another type of relation was predominant. Crozier (6), in a paper on the orientation of animals by opposed beams of light, accounts for Patten's findings in terms of the basic theory of phototropism. In the case of the geotropic orientation of chicks, Hoagland (18) found a linear relation between α and θ, in contrast to the approximately logarithmic one previously discussed for the rats. Kropp and Crozier (20) found a linear relation between θ and sin α for the geotropic orientation of fiddler crabs. In both of these cases the writers have been able to account for their empirical findings in terms of theories of adjustment of excitation.

In some experiments an apparently true logarithmic relation seems to hold between stimulus and response. For example, circus movements of *Limax,* expressed as the amount of turning per unit length of path, has been found to be directly proportional to the logarithm of the light intensity over a fairly wide range [Crozier (8)]. This example, along with other similar ones, in which the orienting power of light is proportional to the logarithm of the intensity and still other cases in which it has been shown to be directly proportional to the intensity has been demonstrated to be in keeping with simple theoretical assumptions consistent with photochemistry (*loc. cit.*).

Again it should be borne in mind that, just as in the case of logarithmic relations, there may be a variety of causes determining *S*-shaped curves. Emerson (12) gets an ogive curve relating the logarithm of light intensity to the amount of photosynthesis. The mechanism underlying this relation can in no wise be related to integral distribution curves. The curve of growth is typically ogival, and a variety of equations have been proposed to account for the function in terms of underlying chemical dynamics [cf. Crozier (7)].

While much of the data relating stimulus and response can probably be accounted for in terms of distributions of thresholds in the nervous system, care should be taken not to draw sweeping generalizations concerning "laws" of excitation. Each case is, in a sense, a special one; and, while logarithmic and approximately logarithmic relations between stimulus and response frequently occur, a further study of the data is usually necessary before sound theoretical conclusions can be drawn.

VI

Under properly selected experimental conditions the curve of learning is also ogival. This is the case when the degree of learning in the experimental material at the beginning is substantially zero, and when the material is such that the amount of it which can be mastered is limited. The ogival nature of the learning curve, most especially in cases of work with nonsense syllables, led Robertson (24) to propose a learning equation similar to that for a monomolecular autocatalyzed chemical reaction. Experiments on the speed of typewriting as a function of time also yield *S*-shaped curves resembling in all respects integral distribution curves [Chapman and Hills (4), Thurstone (26)]. Work of Kleitman and Chrisler (19) on the conditioned reflexes of dogs yields a typical ogival curve which, these workers point out, is better described quantitatively by a normal integral distribution curve than by the curve of Robertson's equation, although they accept Robertson's hypothesis as probable in view of the fact that they can see no way in which an integral distribution curve would be theoretically significant.

Lashley (22) has found, in general, as the result of experiments with rats involving cortical extirpation, that the retention of learned habits depends primarily on the *amount* of remaining cortex and very little on the specificity of cortical areas. Numbers of functioning neurons are correlated by Lashley with degrees of learning. If one accepts this interpretation which is substantiated by other workers with other animals (*loc. cit.*) another physiological basis for the ogival nature of the learning curve suggests itself, a basis which does not require a definite localization of the gross learning process as does Robertson's hypothesis. Learning may be determined by the addition of more and more functional neurons with practice. In this way we may imagine the learning curve to be a measure of the

distribution of thresholds of "synaptic resistances" which are progressively overcome with each repetition of the act to be learned, thus bringing into participation an increasing mass of cerebral tissue. Such an interpretation is consistent with what is known of the reinforcing and tonic effect of the cortex and the higher centers and is also consistent with certain reflex phenomena such as that of irradiation. Plateaus in the learning curve might then be due to the sudden contribution of groups of neurons, as groups, to the learning process. In this way specific centers might contribute en masse. Physiologically, the difference, therefore, between insight learning and other types of learning might be the resultant of a sudden increment of a group of functioning neurons.

If this interpretation of the curve of learning is correct, we have a common quantitative principle uniting both peripheral and central nervous processes which seems to be not inconsistent with what is known at present of nervous-system mechanics.

Summary

Evidence is presented from the fields of vision, pressure sense, and muscle sense, to show that the logarithmic relation known as the Weber-Fechner Law rarely actually exists as such. The apparent relation seems to be an approximation for the mid region of an integral distribution curve relating thresholds of receptors to logarithms of stimulus, or for the range covered by a series of such integral distribution curves corresponding to several separate groups of receptors.

This conception of the so-called Weber-Fechner Law is consistent with the all-or-none nature of nervous-system mechanics. The addition, with increasing intensity of stimulus, of more and more receptors as their thresholds of excitation are reached is a principle which seems to account quantitatively for much of the data usually subsumed under the Weber-Fechner Law.

The learning curve is also ogival under properly selected experimental conditions. It is suggested that this may be due to the participation with practice of increasing numbers of neurons as the thresholds of their "synaptic resistances" are reached. The learning curve may therefore be regarded as an integral distribution curve of thresholds of activation of cortical neurons. This conception is consistent with nervous-system mechanics and receives experimental substantiation from Lashley's work, which correlates learning with the mass of functional cortical tissue.

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LA LOI WEBER-FECHNER ET LA THÉORIE TOUT-OU-RIEN

(Résumé)

On présente de l'évidence des domaines de vision, de sens de pression, et de sens musculaire, pour montrer que la relation logarithmique appelée la Loi Weber-Fechner existe vraiment peu souvent. La relation apparente semble être une approximation pour la mi-région d'une courbe intégrale de distribution rapportant les seuils des récepteurs aux logarithmes du stimulus, ou pour l'étendue d'une série de telles courbes intégrales de distribution correspondant à plusieurs groupes séparés de récepteurs.

Cette conception de la soi-disant Loi Weber-Fechner concorde avec la nature tout ou rien de la mécanique du système nerveux. L'addition, avec l'intensité de plus en plus grande du stimulus, de plus de récepteurs comme leurs seuils d'excitation sont atteints, est un principe qui semble expliquer quantitativement beaucoup des données ordinairement résumées sous la Loi Weber-Fechner.

La courbe d'apprentissage est aussi ogivale dans des conditions expérimentales bien choisies. On suggère que cela puisse être dû à la participation avec exércice de plus grands nombres de neurones comme les seuils de leurs "résistance synaptiques" sont atteints. La courbe d'apprentissage peut ainsi être considérée comme une courbe intégrale de distribution des seuils d'activation des neurones corticales. Cette conception concorde avec la mécanique du système nerveux et est soutenue expérimentalement par l'oeuvre de Lashley, laquelle rapporte l'apprentissage à la masse du tissu cortical fonctionnel.

HOAGLAND

ÜBER DAS WEBER-FECHNER GESETZ UND DIE ALLES-ODER-NICHTS THEORIE

(Referat)

Man führt Beweise aus dem Gesichtsfeld, dem Druckgefühl und dem Muskelsinn an um zu zeigen, dass das logarithmische Verhältnis, als das Weber-Fechner Gesetz bekannt, nur selten als solches besteht. Das scheinbare Verhältnis scheint eine Annäherung des mittlern Gebiets der ganzen Verteilungskurve, welche die Schwellen des Rezipienten mit Logarithmen des Reizes verbindet, oder des Feldes, das von einer Serie solcher integralen. Verteilungskurven bedeckt ist, die mehreren einzelnen Gruppen der Rezipienten entsprechen.

Der Begriff des sogenannten Weber-Fechner Gesetzes stimmt mit der Alles-oder-Nichts Natur des Mechanismus des Nervensystems überein. Das Hinzuziehen, bei zunehmender Intensität des Reizes, von mehr und mehr Rezipienten, so wie die Schwelle der Erregung erreicht wird, ist ein Prinzip, das scheinbar quantitativ für viele der Daten, die man gewöhnlich dem Weber-Fechner Gesetz unterordnet, verantwortlich ist.

Die Lernkurve ist auch ogival unter richtig gewählten experimentellen Bedingungen. Man vermutet dass dies wohl verursacht werde durch die Teilnahme der mit der Übung zunehmenden Auzahl von Neuronen, so bald die Schwellen des "synaptischen Wiederstandes" erreicht sind. Man kann daher die Lernkurve als integrale Verteilungskurve der Schwellen der Erregung der Rindenneuronen betrachten. Diese Auffassung stimmt mit dem Mechanismus des Nervensystems überein, und wird ferner durch Lashley's Arbeit experimentell bestätigt, die das Erlernen mit der ganzen Masse der funktionellen Rindensubstanz korreliert.

HOAGLAND

THE RELATIONS OF ELECTROMYOGRAPHICALLY MEASURED REFLEX TIMES IN THE PATELLAR AND ACHILLES REFLEXES TO CERTAIN PHYSICAL MEASUREMENTS AND TO INTELLIGENCE*†

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The original purpose of the present investigation was to extend the study of the relation between electromyographically measured reflex response latency and intelligence. One of us (Travis) had reported a high correlation between those two variables in the case of the patellar tendon reflex. It was proposed to verify this finding, both for the patellar and Achilles tendon reflexes, and incidentally to discover the correlation between the two reflex times. When it was discovered that the report of correlation between reflex time and intelligence was probably in error, the problem became:

1) To discover whether or not the present methods of recording reflex times are sufficiently reliable to make the results of this and of similar studies dependable.

2) To study the relationships between reflex times and measures of height, length of leg, and length of thigh.

3) To make a final determination as to whether or not there is any relationship between reflex times and intelligence.

An immense amount of research has been done on the tendon reflexes, especially the patellar, since Erb and Westphall first called attention to them in 1871. Most of the work has been of a clinical

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†The importance of this manuscript is obvious to those acquainted with the literature of this problem, and entirely reverses previous findings of the senior author, as published in *The Journal of Experimental Psychology,* 1928, **11,** 342-354. This manuscript was denied publication in the above journal because the Editor felt that his quota of space for reflex psychology had been exhausted. This manuscript is of interest to the laboratories at Harvard, London, Cambridge, Peabody, Clark, Stanford, Iowa, Yale, Columbia, and other places where interest in this problem has been quite keen.—EDITOR

nature, but many studies in pure physiology and physiological psychology have been made. A recent review by Fearing (2) affords a brief summary of this work.

The present survey will confine itself to description of the researches on response latencies in the human being, especially latencies measured by the electromyographic method. As Dodge (1) has pointed out, there have been three methods of recording the reflexes so as to measure their responsive time: (*a*) the recording of muscle thickening, (*b*) the recording of the movement of the limb, and (*c*) the registration of the action currents in the executant muscles. The second method is too crude to merit much attention, since the time is greatly dependent on the length and weight of the limb to be moved. Fearing (2) tabulates the following latent times secured by the first method in the period 1871-1890.

Investigator	Date	Time (sigma)
Tschirjew	1879	58-61
Gowers	1879	15-90
Brissaud	1880	50
ter Meulen		35-90
Waller	1880	30-40
James	1880	25
Eulenberg	1882	24
De Watteville	1882	20
Rosenheim	1884	31-33
Jendrassik	1894	23

In 1909, Franz (3) secured an average latent time of 50 sigma with 495 records in 8 paretic subjects.

The use of recording instruments in which there were considerable and varying amounts of lag doubtless had much to do with the production of such divergent results as these. Dodge (1), in 1911, employing a refined technique, reported 35 sigma for the patellar and 50 for the Achilles. Miles (10), using the same technique, secured similar times, ranging for the patellar from 32 to 35 sigma in normal individuals.

The study of tendon reflex times in man by means of the galvanometric technique began in 1910 and 1911 with the work of Snyder (12), Jolly (8), Hoffmann (6), Dodge (1), and Wertheim-Salomonson (21). The findings of these workers may be summarized as follows.

Investigator	Reflex	Average time (sigma)	Range (sigma)
Wertheim-Salomonson	Patellar	10.0	....
Snyder	Patellar	11.3	8-15
Hoffmann	Patellar	....	19-23
	Achilles	....	31-36
Jolly	Patellar	21.3	20.5-22.7
Dodge	Patellar	14.0	....
	Achilles	35.0	....

These investigations all showed that the latent time was great enough to allow these muscular contractions to be interpreted as reflexes. After the matter of the reflex nature of the jerks was settled, interest in galvanometric measurement of tendon reflex times seems to have died down in all countries except Germany, where both Paul and F. A. Hoffmann have continued it.

In 1883, Alexander James (7) reported on the relation between the rate of ankle clonus and standing height. "Ankle clonus" is the clonic movement of the foot that may be produced by pressing the toe down upon the floor and allowing the knee to move up and down. It is essentially a series of rapidly elicited Achilles reflexes, in which the downward movement of the heel after each reflex serves as a stretch stimulus for the next reflex. James found that the mean of a group of 10 men, ranging from 6 ft. to 6 ft. 4 in. in height, was 5.84 clonic contractions per second, an average of .171 seconds per clonic movement. In 9 individuals, ranging from 5 ft. to 5 ft. 4 in., the mean was 6.91 per second, an average of .145 seconds per clonic movement. This gives a difference between the groups of .026 seconds per movement. James ascribed this difference to the added length of the reflex arc. On the basis of the data we shall report, the difference due to length of arc should be scarcely greater than .010. The remaining difference must be due to other factors.

In 1916, F. A. Hoffmann (4) studied the relation of patellar reflex time, as measured by the thread galvanometer, to length of reflex arc. He found, in cadavers, a close relationship between height from head to heel and length of arc, and, in living subjects, a close relationship between reflex time and standing height. His times ranged from 14 sigma in children to 26 sigma for normal adults. He showed that the rate of conduction changes with vari-

ous pathological conditions, notably diabetes, where it is considerably slower than in health.

In 1918 (5), he reported a more extended study of the Achilles reflex. After working with about 100 cadavers, he was able to show that there is a very close, practically linear, relationship between length of arc and height. He measured the arc from a point just below the twelfth dorsal vertebra, not from the point at which the nerve enters the cord between the fourth and fifth lumbar vertebrae. From Hoffmann's chart, we have calculated that the arc, as we have measured it, is about 1.14 times the height of the individual. Taking the height, Hoffmann calculated the length of arc, and then, with his measure of reflex time, calculated the rate of conduction throughout the arc. In 15 normals, he found the reflex time to range between 26 and 44 sigma, with a mean of 35.1 sigma. The mean rate of conduction was 54.7 meters per second, with a range extending from 48.6 to 65.5. The average length of arc was 192 cm., and the average height of the individuals (according to our calculations), 168.4 cm.

He found the rate of conduction somewhat dependent on height, age, amount of sugar in the blood, and temperature.

Since 1927, the senior author of the present study has been doing a considerable amount of electromyographic measurement of reflex times by amplification methods in the psychology laboratory of the State University of Iowa. In the main he has used two types of apparatus. The first type employed was a five-stage amplifier activating a recording device in the form of an optical lever, known as the *Phonelescope*. The second apparatus developed is the one employed in this study and described later. It is found to be much more reliable than the Phonelescope set-up. The average times for patellar reflex in normal adults are found to be between 18 and 20 sigma with both these devices. These times are somewhat shorter than those reported with the thread galvanometer, probably because there is considerable lag in the thread of the latter device.

The variations found with the Phonelescope (20) are about the same as those reported with the galvanometer, but are greater than those found in normals with the oscillograph. We believe that the oscillographic measurements show less variation because they are more reliable. Travis (14, 15, 16) reports considerable variation in reflex times measured by the oscillograph in various pathological cases.

Tuttle (18), using Travis' oscillograph set-up, reports Achilles reflex times ranging from 30 to 40 sigma; but he has read the time to the crest of the first deflection on the record rather than to the beginning of the deflection, as has been the usual practice. He reports that increase in strength of stimulus, changes in rate of stimulation, and re-enforcement do not materially affect reflex times.

This last report is contrary to the statement of F. A. Hoffmann (4) that re-enforcement shortens the time. Hoffmann attributes his shortening to the fact that facilitation increases the amplitude of deflection. He states that relatively large deflections show relatively short times. He believes that this effect is due to inertia in the thread of the galvanometer.

In 1928, Travis (13) and Travis and Hunter (17) reported a high correlation between reflex time in the patellar reflex and intelligence. Two groups were used in these studies, a group of college freshmen, with which the composite score of the Iowa Entrance Examination was used as a measure of intelligence, and a miscellaneous group, with which the Otis Self-Administering Test, Higher Examination Form A, was used as the measure.

There were slightly over 40 subjects in each group, and the correlation between the *shortness* of the reflex time and intelligence was .87 for both groups.[1]

The apparatus finally developed in their experiment was the same that was used in the present study. But the majority of their data were secured with the Phonelescope.

Apparently in line with this report was the one made by Rounds (11) at about the same time. He measured the muscle thickening time in the Achilles reflex and correlated it with a group of speed tests, securing correlations of about .60 with combined scores on the tests. One of Rounds's conclusions fairly indicates his interpretation of his findings:

> "The latent time reveals a certain level of speed in that part of the nervous system which subserves the reflex. The correlation ratio points to a similar level of speed of transmission in other parts of the nervous system of a given individual."

It should be apparent that the results of F. A. Hoffmann and

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[1]Attention is called to the fact that throughout the present investigation correlations are made with *length* of reflex time. On this basis, the coefficients reported by Travis and Hunter would be —.87.

those of Travis and Hunter, and possibly of Rounds, are mutually exclusive. The German investigator states that individual differences in reflex latencies are due almost entirely to differences in length of arc. If this is true, there can be no very high correlation between intelligence and reflex time, since it is well known that there is little relationship between height and intelligence.

Apparatus and Methods

The apparatus used in this study is the same as that described by Travis and Hunter (17). Briefly, it consists of a three-stage, resistance-coupled amplifier, a portable three-element oscillograph, a vacuum-tube oscillator, and a signal circuit.

The amplifier furnishes medium amplification and, as determined by controlled tests, is practically free from inherent disturbances. The oscillograph is manufactured by the Westinghouse Electric and Manufacturing Company. We used two super-sensitive elements each capable of responding within 1/20,000 of a second and one standard element capable of responding within 1/30,000 of a second. One of the super-sensitive elements was used for recording the electrical changes in the muscle and the other for recording time. The standard element was used to record the instant of striking the tendon. A special photographic unit was devised to replace the one with which the oscillograph was originally equipped. This unit handles 400 feet of standard-size moving-picture film. The oscillator, a General Radio Company low-frequency oscillator, type 377, was used to furnish a time line of 1000 complete cycles a second. The stimulus circuit was actuated by discharging a condenser that had been charged previously. The condenser discharged through a resistance, a vacuum tube, and a transformer. The electrodes were thin brass strips, ½ by 3 inches. They were covered by Canton flannel moistened with saturated salt solution. For the patellar reflex the first electrode was placed on the executant muscle half way between the anterior-superior spine of the ilium and the lower margin of the patella, and for the Achilles reflex on the executant muscle one-fifth of the way between the lower margin of the patella and a point even with the inferior malleolus. In both cases the second electrode was placed about one-half inch distal to the first. The time elapsing between the closing of the signal circuit, upon the instant that the tendon is tapped, and the ap-

pearance of the reflex action current volley can be read on the time line in terms of .5 sigma without interpolation.

A diagram of the apparatus for stimulation in the Achilles reflex is shown in Figure 1. A pendulum *A*, falling on a stimulus rod *B*, furnishes a constant stimulus. The head of the rod *B* is held against the Achilles tendon by rubber bands *F* which are fastened to the board *G*. The leg is held firmly against this board by the strap *J*. The strap *H*, attached to the elastic band *I*, exerts pull on the foot sufficient to stretch the tendon slightly. The stim-

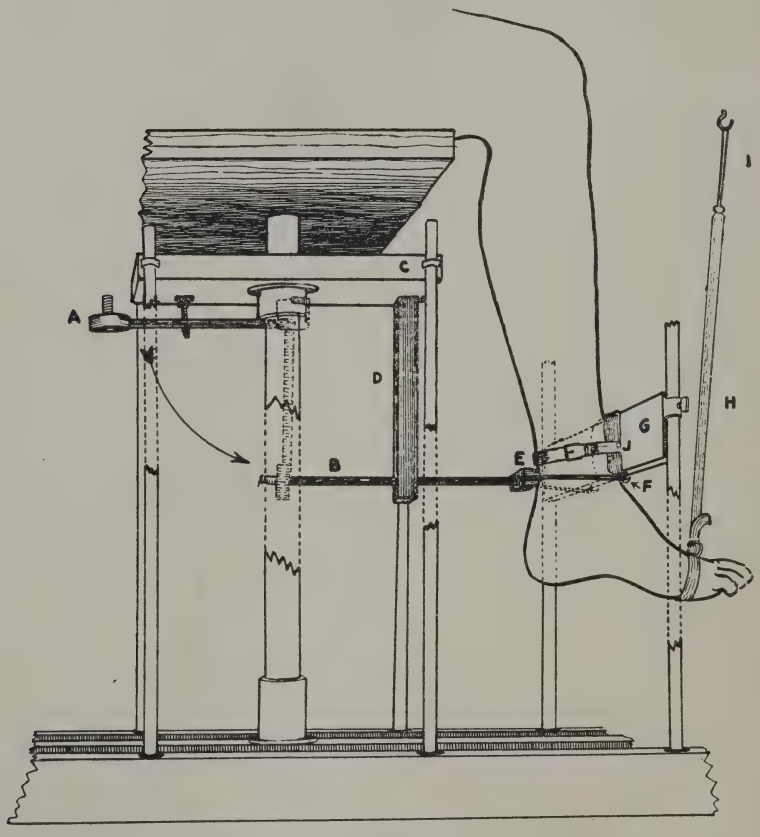

FIGURE 1
APPARATUS FOR ELICITING ACHILLES TENDON REFLEX
Description in text

ulus rod is held in place by passing loosely through a hole in the bar *D*. *D*, along with the pendulum, is bolted to the vertically adjustable platform *C*. The front board *G* is also vertically adjustable.

After some trial, it was found to be more difficult to elicit the patellar reflex by means of a constant stimulus than by the method in current use in the laboratory, namely, striking the tendon with a steel mallet. Since Tuttle (20) had shown that strength of stimulus does not appreciably affect the reflex time, it was felt that there was no reason for using some less convenient constant stimulus method rather than the hammer.[2]

In the case of the Achilles reflex, the signal circuit was made upon the contact of the pendulum hammer with the stimulus rod. For the patellar, it was made by contact of the mallet upon a brass strip that had been fastened across the tendon at the stimulation point.

Typical records are shown in Figures 2 and 3. The time elapsing between the signal mark and the first deflection of the action current volley is counted off to the nearest .5-sigma. From 6 to 10 reflexes are taken for an individual in a single sitting. Readings are taken on all of those that are clearly readable, and the mean of the readings is calculated. This figure is accepted as the nearest approximation to the true reflex time of that individual.

Data

The attempt was made to secure records of reflex times on approximately 150 members of the freshman and sophomore classes of the University of Iowa. From these 122 readable records of patellar times and 119 of Achilles times were secured. Records of both times were secured on 111 individuals.[3]

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[2]That a constant stimulus is not necessary for the accurate measurement of electromyographically determined reflex times is demonstrated by our results. The reliability coefficients (see below) for the Achilles reflex with which a constant stimulus was used are higher than those for the patellar reflex, but this is because of the greater variability in the Achilles reflex. For comparison of the accuracy of measurement on a given individual, the probable error of the score is the significant measure. The probable error of the score for the patellar reflex is lower than it is for the Achilles.

[3]The chief reasons for failure to secure readable records were: (1) damage to the film; (2) outside electrical disturbances; and (3) absence of or extremely weak jerks.

For each individual, a measure of standing height and of length of thigh was secured. The measure of the thigh was taken from the anterior superior crest of the ilium to the lower margin of the patella. On 94 individuals the distance from the lower margin of the patella to a point even with the inferior malleolus was measured.

Composite scores on the Iowa Entrance Examination and scores on the comprehension test included in that examination were secured for each of the subjects.

In addition to the data on the University group, we secured the following data:

1) Patellar records on 47 fifth- and sixth-grade pupils, 47 second- and third-grade pupils, and 37 preschool children.

2) Measures of height on all these pupils.

3) Stanford-Binet *I.Q.*'s for the grade school children and Kuhlmann *I.Q.*'s for the preschool children.

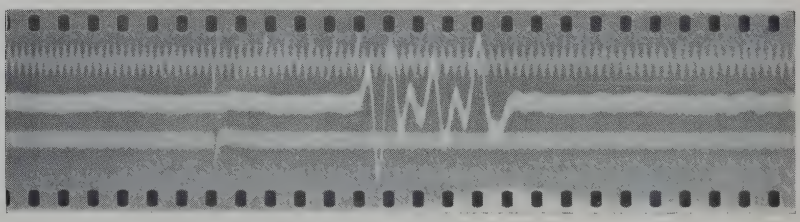

FIGURE 2

TYPICAL PATELLAR TENDON REFLEX RECORD

The marks used in reading the record can be seen along the time line. The reflex time in the record was read as 17.0 sigma

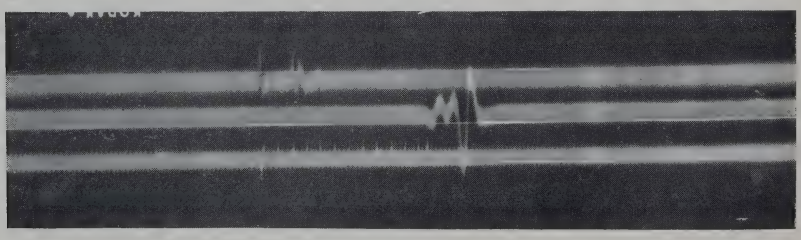

FIGURE 3

TYPICAL ACHILLES TENDON REFLEX RECORD

The first deflection in the signal line indicates the instant of striking the tendon. The reflex time in the record was read as 35.5 sigma

In connection with the study on reliability some further additional data were secured. They will be reported later.

Treatment of the Data

Average Reflex Times and Their Variation. Tables 1 and 2 show the means, standard deviations, and ranges for the times of the two reflexes in all groups.

These times are slightly shorter than those usually recorded by galvanometric techniques. They average the same as those secured by the Phonelescopic technique. The chief point of difference is that, after the probable height of the individuals measured is taken into consideration, there is considerably less variation in the present figures. If we had attempted to determine the times on certain records which weren't sufficiently objective, it is probable that our report would contain nearly as great a range as that reported by F. A. Hoffmann, or that reported with the use of the Phonelescope. We believe that the method of the present study has given a truer estimate of variation *in normals,* since the refinement of the technique has permitted the detection of erroneously long or short times by making more apparent the distinction between valid and invalid records.

Correlation between the Two Reflex Times. The correlation coefficient between the two reflex times for 111 cases (entire university group) was $.787 \pm .043$. This indicates that the response latencies in these reflexes are dependent on the same or correlated factors.

Reliability of the Reflex-Time Measurements. The first measures of reliability secured were made on a group of six persons, on each of whom recordings were made in eight different sittings. Six to ten records of each reflex were secured for each individual in each sitting. The first series was compared with each of the seven suc-

TABLE 1

Reflex Times (in Sigma) for the Achilles Reflex

Group	Mean	*S. D.*	Range
Entire university	31.04	2.142	26.0-36.5
University men	32.32	1.740	28.8-36.5
University women	30.01	1.812	26.0-35.3

TABLE 2

REFLEX TIMES (IN SIGMA) FOR THE PATELLAR REFLEX

Group	Mean	*S. D.*	Range
Entire university	18.93	1.276	15.2-21.6
University men	19.88	1.128	17.2-21.6
University women	18.22	.900	15.2-20.6
5th and 6th grade	16.71	.960	14.7-19.1
2nd and 3rd grade	15.26	.870	14.8-16.8
Preschool	13.49	.783	11.7-15.9

TABLE 3

RELIABILITY COEFFICIENTS FOR REFLEX TIME MEASURES, TOGETHER WITH STANDARD DEVIATIONS AND PROBABLE ERRORS OF THE MEASURES

Group	Reflex	Reliability coefficients	*S. D.* of measures (in sigma)	*P. E.* of measures (in sigma)
Group of 6	Achilles	.93±.016	2.47	.440
Group of 6	Patellar	.86±.031	1.34	.338
Sophomore Group of 39	Achilles	.90±.026	1.87	.399

TABLE 4

VARIATIONS IN SUCCESSIVE MEASURES OF REFLEX TIMES

Group	Reflex	*M. V.* (in sigma)	Range of variation (in sigma)
Group of 6	Achilles	.64	0.0-2.0
Group of 6	Patellar	.59	0.0-1.8
Sophomore Group of 39	Achilles	.85	0.1-1.8

TABLE 5

VARIATIONS (IN SIGMA) IN A SINGLE SERIES OF RECORDS

Reflex	Greatest *M. V.*	Least *M. V.*	Average *M. V.*	Greatest range	Least range	Average range
Achilles	.72	.00	.35	3.5	0.0	1.4
Patellar	.87	.00	.41	3.5	0.0	1.6

ceeding series for purposes of correlation. Since the smallness of these groups might cause an error in selection, the Achilles measures were repeated, for purposes of measuring reliability, on 39 members of the sophomore group. The results are shown in Table 3.

Table 4 shows the means and ranges of the variations in reflex time for the groups upon which the measures were repeated.

The variability in a single series of records was determined for both the patellar and Achilles reflexes from the records of 60 adult individuals chosen at random. The results are shown in Table 5.

Correlations between First and Second Halves of Records. The correlations between the first and second halves of records taken at a single sitting are very high. For 84 individuals in the Achilles reflex, where 10 records were made at each sitting, the correlation between the first five and the second was .98. For 86 individuals in the patellar reflex, the correlation was .94. These coefficients are about equal to the coefficients between two readings of the film. This fact does not mean that there are no true variations in a single set of records. The size of the coefficients is due in part to the influence one reading has on another.

In view of these high correlations between the two halves, it is not worth while to make 10 records at a single sitting. We believe that four Achilles records and six patellar records should give a sufficiently adequate sampling.

Relation Between Reflex Times and Physical Measures. Table 6 shows the correlations between standing height and length of latent response times for the various groups that we have studied.

On the university group measures calculated to be more direct functions of the reflex arc than height were secured. Tuttle and

TABLE 6

CORRELATIONS BETWEEN HEIGHT AND REFLEX TIMES

Group	Reflex	Correlation with height
Entire university	Achilles	.847±.018
Entire university	Patellar	.730±.029
5th and 6th grades	Patellar	.704±.042
2and and 3rd grades	Patellar	.500±.074
Preschool	Patellar	.531±.080

TABLE 7

CORRELATIONS OF REFLEX TIMES WITH ADDITIONAL PHYSICAL MEASUREMENTS —UNIVERSITY GROUP

Measures correlated	Correlation
Patellar time with length of thigh	.690±.032
Achilles time with length of thigh	.738±.028
Achilles time with twice length of thigh plus length of shin	.766±.029

MacEwen (20) have reported, from a study on about 20 cadavers, that the distance from the spinal cord roots of the femoral nerve—between the fourth and fifth lumbar vertebrae—to the motor point of the thigh (half way between the upper margin of the patella and and anterior-superior spine of the ilium) is equal to the length of the thigh. The reflex arc, then, traverses the length of the thigh from its distal end to a point in the spinal cord which is supposed to be only a few inches above the spinal roots, and back to the point at which the electrodes are placed. The arc should be about two and one-half times the length of the thigh. If this supposition is correct, correlations with length of thigh should be as high as correlations with direct measures of length of arc, since the former should be a direct function of the latter.

In a similar way, a more direct function of the Achilles arc than that secured by measuring height was sought by taking twice the length of the thigh plus the length of the shin.

With these additional measures the correlations shown in Table 7 were secured.

The correlations throughout are lower than those with height. This result may be due to the fact that height is more reliably measured than the length of the thigh. On the other hand, addition of length of shin to length of thigh in correlating with the Achilles times increases correlation, although length of shin is probably less reliably measured than thigh length, and the standard deviation of the Achilles measures on the thigh-shin group drops from 2.142 to 1.746 sigma. (Shin measures were secured on only 94 subjects.)

For one measurement, at least, even probable loss of reliability does not lower the correlation when a more direct function of the arc is secured. Probably all the physical measures are so reliable that any difference in reliability is inappreciable. These considerations raise the question: Is height a closer function of the true length of arc in these reflexes than is a direct measure of the length of the peripheral arc?

These correlations between height and reflex times bear out the findings of F. A. Hoffmann. Apparently, the two reflex times correlate with height *almost to the extent of the reliability of their measurement.* To put the foregoing statement to further test, the Achilles reflex was correlated with height for those 39 individuals

upon whom a second measure of reflex time was taken. It will be recalled that the correlation between the two measures was .879 for this group. Table 8 shows the correlations for the same group between height and the first measure, height and the second measure, and height and the mean of the two measures.

These results suggest that the measures made on the group selected to test reliability probably were not as reliable as those made on the group as a whole. They also indicate that the correlation with height is not as close to the reliability of the test as it appears if the correlations with height for the entire group are compared with the reliability coefficient for the group of 39. Height itself, however, cannot be a perfect function of length of arc, and we must conclude that if such a perfect measure were made it would come very close to correlating with reflex time to the extent of the reliability of measurement in the latter. There is still room for *some* individual variations in rate of conduction over the arc.[4]

We thought that there may be some significant differences in reflex conduction rates between individuals of different ages. Since reflex time correlates higher with standing height than with a sup-

TABLE 8

CORRELATIONS BETWEEN HEIGHT AND REPEATED MEASURES OF ACHILLES TIME—SOPHOMORE GROUP OF 39

Measure	Correlation
1	.715±.053
2	.766±.045
Means of 1 and 2	.790±.040

TABLE 9

AVERAGE HEIGHTS AND PATELLAR TIMES, AND THE RATIOS BETWEEN THESE

Group	Height (in cm.)	Patellar time (in sigma)	*H/T*
University	165.88	18.93	8.76
5th and 6th grades	143.50	16.71	8.59
2nd and 3rd grades	128.95	15.26	8.45
Preschool	106.91	13.49	7.93

[4]There may be measurable differences between tall and short individuals in the time elapsing between the striking of the tendon and the stretching of the muscle, a time which would give the taller person an apparently slower conduction rate. But inasmuch as reflex time correlates higher with height than with length of thigh this difference must be very small.

posedly more direct measurement of the length of the arc, the ratio height divided by reflex time (H/T) was used as the best function of conduction rate and consequently as the most satisfactory unit for group comparisons. Inasmuch as we did not obtain Achilles records on our children, the comparisons are made on the basis of the patellar only. Table 9 gives these comparisons.

Here we find that the longer the arc, the greater is the apparent rate of conduction. This is what would naturally be expected if rate of conduction over the nerve-trunk was constant and if end-organ, synaptic, and end-plate latencies were the same for all individuals. In such a case, the various constant latencies would form a greater proportion of the reflex time in shorter arcs than in longer ones, and the apparent rate of conduction would be slower in the shorter arcs.

Correlation between Reflex Times and Intelligence. In this study no definition of intelligence is offered save that it is what the intelligence tests measure. An intelligence test, for our present purpose, is merely one which correlates highly with other intelligence tests. The test that has been used is the Iowa Entrance Examination (1927), which has a reliability coefficient of .95. It included the Iowa High School Content Examination, the Iowa Comprehension Test, the Iowa Placement Examination (English Training), and the Iowa Placement Examination (Mathematics Aptitude).

The chief purpose in the construction of this examination has been to predict scholastic achievement, not to measure intelligence. Two of the tests which compose it were constructed for the purpose of predicting achievement in special fields, English and mathematics. But these are no reasons for expecting the Examination to be a poorer test of intelligence than any other battery. Kelley (9) points out that so-called "educational achievement tests" test much the same thing that so-called "intelligence tests" do.

Table 10 shows correlations between scores on the Thorndike Intelligence Test for High Schools and Colleges and the tests given at Iowa in 1927 and 1928. One hundred cases were used for each correlation.

No correlations with other tests have been secured for the composite.

In order to compare the results on the composite test with those secured by using a test not attempting to measure specific abilities,

the measures of reflex time were correlated not only with the composite but also with the Iowa Comprehension Test, which forms a part of it. Since the comprehension test is a part of the composite, the correlation between the two is somewhat higher than it would be otherwise. It is .860±.015 for the group with which this study is concerned. The reliability of the comprehension test is .85.

In our groups the frequency distributions for the intelligence tests

TABLE 10

CORRELATIONS BETWEEN TESTS COMPOSING THE IOWA ENTRANCE EXAMINATION AND THORNDIKE SCORES

Test	Correlation with Thorndike scores
English Training	.461
Mathematics Aptitude	.601
Comprehension	.702
High School Content	.734

TABLE 11

MEANS, STANDARD DEVIATIONS, AND RANGES FOR INTELLIGENCE TESTS

Test	Group	Mean	*S.D.*	Range
Composite	University	380.81	102.00	179-611
Comprehension	University	19.55	7.46	4-39
Stanford-Binet	5th & 6th grades	116.25	14.85	89-143
Stanford-Binet	2nd & 3rd grades	118.56	15.26	88-148
Kuhlmann	Preschool	113.57	12.56	88-147

all tended toward the normal distribution, and there was a sufficiently wide range of distribution in all cases to allow for correlation if correlation existed. On the university group, the composite scores ranged from the third to the 99th percentile of the entering class, and the comprehension scores from the first to the 99th.

The means, standard deviations, and ranges for the intelligence tests used with the various groups are shown in Table 11. The figures for the school children are in terms of *I.Q.*, and those for the university group in terms of raw score.

The correlation coefficients secured between reflex times and the measures of intelligence are shown in Table 12. High negative coefficients in this table would support the finding of high correlation between rate of neural conduction and intelligence.

Insignificant as these coefficients are, they become even smaller

when height is partialled out. Table 13 shows the partial coefficients.

Correlations between H/T and intelligence for the various groups are given in Table 14.

These results are entirely contrary to those reported by Travis and Hunter (17), giving the correlations between intelligence and length of reflex time as —.87. (They report it as .87 between intelligence and speed of the reflex.)

Since the two sets of results were secured by identical procedures, with the exception of the fact that most of the data in the former study were secured on the Phonelescope, the contrast in results is the more marked. The present results also do not appear to accord with those of Rounds (11). But it should be remembered that Rounds used entirely different measures of reflex time and of mental ability from the ones we have used. Rounds's "speed tests" would not be likely to show much correlation with intelligence tests. That his measures of reflex time are not at all comparable to the ones herein reported is demonstrated by the fact that the variability of

TABLE 12

CORRELATION COEFFICIENTS BETWEEN REFLEX TIMES AND MEASURES OF INTELLIGENCE

Group	Reflex	Entrance examination	Comprehension test	Stanford-Binet	Kuhlmann
University	Achilles	.037±.062	.058±.062		
University	Patellar	.074±.061	.045±.061		
5th & 6th grades	Patellar			—.063±.096	
2nd & 3rd grades	Patellar			—.084±.090	
Preschool	Patellar				.237±.105

TABLE 13

PARTIAL CORRELATION COEFFICIENTS BETWEEN REFLEX TIMES AND INTELLIGENCE MEASURES, HEIGHT HELD CONSTANT

Group	Reflex	Entrance examination	Comprehension test	Stanford-Binet	Kuhlmann
University	Achilles	.044	.011		
University	Patellar	.037	.035		
5th & 6th grades	Patellar			.118	
2nd & 3rd grades	Patellar			—.071	
Preschool	Patellar				.154

his measures is so much greater. His reflex times were secured on a group of university men. The standard deviation of his group was 13.35 sigma, and the range 65 sigma. The standard deviation of reflex time for our group of university men was 1.74 sigma, and the range 7.7 sigma. The variation in the electromyographically measured time, therefore, could have little effect in producing the variation measured by Rounds, and there is no reason for supposing that the two measures would be highly correlated.

The data presented in this study are of such a nature that the evidence for extremely low correlation between intelligence test scores and reflex times seems well nigh incontrovertible. The number of cases is far too great in every instance to allow for the result being due to chance. And the same results have been secured for a university group of 120 subjects, on both the Achilles and patellar reflexes, and for three groups of school children, the latter groups totalling 131 subjects.

Furthermore, the reliability of the measures is too great to permit a low correlation because of low reliability. Repeated reflex measures give the same results that the first ones did, while the mental tests used are among the most reliable to be had. The consistent high correlations with height show again that failure of the reflex measures to correlate with intelligence is not due to inability to correlate because of low reliability.

Finally, our finding, in agreement with those of former workers, that the rate of neural conduction in the arc is nearly constant and that individual differences in reflex time are largely dependent on differences in length of arc, would make it impossible for reflex time to be highly correlated with intelligence, or with anything not correlated with length of arc.

In the face of such strong evidence, it became necessary to look

TABLE 14

CORRELATION COEFFICIENTS BETWEEN H/T AND INTELLIGENCE

Group	Reflex	Entrance examination	Stanford-Binet	Kuhlmann
University	Achilles	.043		
University	Patellar	—.027		
5th & 6th grades	Patellar		—.091	
2nd & 3rd grades	Patellar		.063	
Preschool	Patellar			—.050

TABLE 15

CORRELATION COEFFICIENTS OF NEW READING OF FRESHMAN GROUP RECORDS WITH ENTRANCE EXAMINATION SCORES AND WITH FORMER READINGS

Group	Entrance examination	Former readings
Group of 39	—.027±.108	.322±.097
Group of 28	.043±.108	.489±.096

carefully into the data which had indicated such astoundingly high correlation between intelligence and reflex time. The film records for the miscellaneous group of Travis and Hunter's study were not readily available; but those for the freshman group were on file in the laboratory. These records, which were all secured with the Phonelescope, were carefully re-read, without knowledge concerning the persons on whom they had been secured.

The reading of these records revealed the fact that it was much more difficult to get a positive reading of the reflex time on them than on the oscillograph records. The readers were not at all sure of many of their judgments. Reflex times on 28 of the individuals, however, seemed fairly certain. These were put in a separate group. The records on 11 others seemed to furnish some indication of reflex time, and these were added to the group of 28 to form another group. Table 15 indicates the correlations of these readings with Entrance Examination scores and with the readings on the basis of which correlations with intelligence had been reported.

Astounding as the complete failure to find correlation is, it is hardly more remarkable than the contrast between the re-reading correlations with this film and those found on the oscillograph film, all of which were over .90. Using the more easily read film only, the correlation between two readings increases from .322 to .489, but the tendency toward high negative correlation with intelligence certainly does not increase.

The discrepancy between our results and those reported in the preliminary study is apparently due to the fact that the records of reflex time secured in the former study could not be read with sufficient accuracy to secure reliable results.

ANALYSIS OF POSSIBLE ERRORS IN MEASURING REFLEX TIMES

The accuracy of the measures used in this investigation is doubtless high. Nevertheless, we have undertaken to attempt an analysis

of certain possible errors, to measure them, and then to suggest methods for their elimination.

Reading. The error which we were most concerned to measure, in order to determine whether or not it had an appreciable effect upon results, was that which might be introduced by unreliable or biased readings. The chief reason for this concern was that it appeared that the records secured by use of the Phonelescope were not capable of sufficiently accurate reading to give reliable results. During the present investigation, precautions were taken to avoid bias. We did not know at the time of reading the film what were the intelligence scores of the subjects. In most cases, we did not know whose records we were reading. It was, however, impracticable to arrange the investigation so that we would not know that we were reading the record of a man or a woman, and this introduced expectancy of shorter times for women than for men.

In order to determine whether or not our readings were reliable, we removed, after the first reading, all names from the film and substituted numbers, so that it was impossible for us to know whose film was being read. All guide marks made during previous readings were removed. We then re-read 110 records of each reflex in complete ignorance of our former readings. The marks made during our second readings were removed, and two research assistants, A and B, re-read a total of 60 Achilles records and 61 patellar records. Tables 16 and 17 give the correlations between these readings.

Our second readings are the least like those of the other readers while the correlations are highest between our first readings and A's and B's readings. This indicates certainly that there is little individual idiosyncrasy in reading. Our first readings are the most nearly like those of the others, indicating that they could have contained very little bias.

Fluctuations in the Time Line. We thought that the time line furnished by the General Electric low frequency oscillator might not be perfectly constant. It was difficult, however, to find any instrument capable of giving a *perfect* time line with which to make comparisons. It was finally decided to check simply by recording 60-cycle current along with the 1000-cycle oscillations that had been used. It is claimed by engineers at the local power-house that oscillations in the 60-cycle current do not vary more than eight-tenths of one per cent during the course of a day.

The 60-cycle current and the 1000-cycle oscillations were photographed simultaneously on eight different afternoons. The number of 1000-cycle oscillations for 24 60-cycle oscillations was read. If the two oscillations were exactly accurate, there would be 400 1000-cycle oscillations to 24 60-cycle oscillations. The results secured are shown in Table 18.

TABLE 16

CORRELATION COEFFICIENTS BETWEEN RE-READINGS OF ACHILLES RECORDS

	Author's first reading	Author's second reading
Author's second reading	.970±.003	
A's and B's reading	.982±.003	.966±.066

TABLE 17

CORRELATION COEFFICIENTS BETWEEN RE-READINGS OF PATELLAR RECORDS

	Author's first reading	Author's second reading
Author's second reading	.945±.007	
A's and B's reading	.949±.009	.913±.014

TABLE 18

NUMBER OF 1000-CYCLE OSCILLATIONS PER 24 60-CYCLE OSCILLATIONS

Date	Number
April 3	395.0
3	394.5
4	395.0
9	393.5
9	394.0
11	392.0
16	393.0
18	391.0
May 3	390.0
8	392.5

The total fluctuation is 1.25%. This might be entirely accounted for by fluctuation in the 60-cycle current.

The investigation of this possible error is not as complete as it should be. No evidence, however, has been found to indicate that a very important error is introduced by variations of the time line. A variation of 1% in the time-line frequency would produce an error of only .2 sigma for the patellar reflex and .3 sigma for the Achilles.

Variation in Amount of Deflection. Both Paul and F. A. Hoffmann (6, 4) have noted that relatively large deflections of the action-current line are associated with relatively short response times. The same association has been discovered in the present study. The German investigators attribute this difference solely to the inertia in the galvanometer string. To determine whether or not inertia in the oscillograph element was responsible for the longer times associated with slight deflections, the following experiment was undertaken.

The oscillograph includes a variable resistance in series with each of the elements. A series of 40 reflex records was taken on an individual, and, with every five records, the resistance in series with the element recording the action current was shifted from minimum to maximum, or vice versa. In this way, the potential across the element was varied without any similar variation in the intensity of the physiological action current.

This experiment was performed five times, twice on each of two

TABLE 19

DIFFERENCES BETWEEN MEANS OF RESPONSE TIMES WITH MAXIMAL AND MINIMAL RESISTANCE IN SERIES WITH ELEMENT
(All figures in terms of sigma)

Subject	Mean minimal resistance	Mean maximal resistance	Difference	$\frac{Diff.}{P.E._{diff.}}$
1	35.425±.043	35.565±.043	.140±.061	2.30
2	28.200±.066	28.925±.043	.725±.079	9.18
2	27.882±.080	28.464±.087	.582±.118	4.93
3	30.750±.061	31.026±.053	.276±.081	3.41
3	31.051±.045	31.333±.037	.282±.058	4.86

TABLE 20

DIFFERENCES BETWEEN FIRST AND LAST 20 RECORDINGS IN SERIES OF 40 RECORDINGS

Subject	Difference (in sigma)	$P.E._{diff.}$* (in sigma)	$\frac{Diff.}{P.E._{diff.}}$
1	.121	.062	1.95
2	.141	.054	2.61
3	.121	.062	1.95

*Probable errors were calculated by using standard deviations of entire distributions of 40. They are, therefore, not quite accurate.

subjects and once on another. Table 19 shows the results of this study in terms of means, differences, and ratios of differences to their probable errors.

Although they are very small, the differences here are significant. But the differences in extent of deflection on the action-current line brought about by varying the resistance were also quite small, and there can be no doubt that the inertia of the element may cause much larger errors than those here demonstrated. Whether or not the action current actually appears in the muscle sooner in the case of strong responses than in the case of weaker ones has not been determined.

Variability in Stimulation. Tuttle (18) has reported that there is no change in reflex time—as measured by the oscillograph—conditioned by change in the strength of the stimulus. Since the opportunity was presented in this experiment to accumulate many more records than were secured by him, it was decided to see if his finding held good if small changes in a large body of data were studied. For half the group the weight of the pendulum was approximately doubled during the final half of the recording for the Achilles reflex, and for the other half no weight was added. For the group in which the change was made, the mean of the second half of the records was .1018±.0192 sigma less than the mean of the first half. For the group in which the stimulus was held constant, the mean of the second half of the records was .1113±.0331 sigma more than the mean of the first half. The difference between these variations is .2131±.0383 sigma. The reflex times with the added weight are slightly, but quite significantly, shorter than those without it. The difference is nearly six times its own probable error, so that it cannot be due to chance. It may be due to two factors: (*a*) to a stronger response which produces a greater deflection of the oscillograph element, and (*b*) to a reduction in the time which may elapse between the contact of the signal hammer with the metal plate and the stretching of the muscle to stimulate the afferent endings. Both of these factors, in turn, would be due to the increase in the strength of the stimulus. Thus the apparent reduction in reflex time due to an increase in the strength of the stimulus may be accounted for along mechanical lines.

Effect of Continued Elicitation. When no weight was added, the last half of the records averaged .1113±.0331 sigma longer than

the first half. The ratio of this difference to its probable error is 3.6, not quite high enough to be indubitably significant. To test the matter further, the data secured for studying the effect of change in resistance were used. The first 20 records in each sitting were contrasted with the last 20. The differences, their probable errors, and the ratio between them for each of the three subjects are shown in Table 20.

The consistency of these results indicates that the response time increases slightly but significantly with continual elicitation of the reflex. Very likely this difference is also a special instance of the correlation between shortness of recorded time and amplitude of action-current deflection.

Summary and Conclusions

1) By means of a Westinghouse Portable Oscillograph we secured electromyographic determinations of reflex response latencies with a reliability coefficient of about .90 for the Achilles reflex and .80 for the patellar reflex on a group of young adults of both sexes.

2) The records were read with a reliability of about .97 for the Achilles reflex and .94 for the patellar.

3) The probable error of the reflex time as secured from a single series of from 6 to 10 records is approximately .4 sigma for the Achilles and .3 sigma for the patellar reflex.

4) The correlation between the Achilles and the patellar reflex times is .79.

5) Reflex times show high correlations with physical measures that are calculated to be functions of the length of reflex arc. The highest correlations found are those between reflex times and height. They are .85 for the Achilles reflex and .73 for the patellar in the group of university underclassmen.

6) On the basis of reflex-time records from 250 individuals, ranging in ages from 4 to 35 years, no evidence is found for correlation between reflex time and intelligence.

7) On the same basis, no evidence is found for correlation between rate of neural conduction in the reflex arc and intelligence.

8) The data on the basis of which correlations between reflex time and intelligence were previously reported from this laboratory have been shown to be unreliable.

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LES RELATIONS DES TEMPS DE RÉFLEXES MESURÉS ÉLECTROMYOGRAPHIQUEMENT POUR LES RÉFLEXES PATELLAIRE ET ACHILLÉEN À CERTAINES MESURES PHYSIQUES ET À L'INTELLIGENCE

(Résumé)

Au moyen d'un Oscillographe Portatif Westinghouse on a obtenu des déterminations électromyographiques des latences des réponses de réflexes avec un coefficient de constance d'environ 0,90 pour la réflexe achillén et de 0,80 pour le réflexe patellaire chez un groupe de jeunes adultes des deux sexes.

On a lu les résultats avec une constance d'environ 0,97 pour le réflexe achilléen et de 0,94 pour le patellaire. L'erreur probable du temps de réflexe comme obtenu d'une seule série de 6 à 10 résultats est approximativement de 0,4 sigma pour l'achilléen et de 0,3 sigma pour le réflexe patellaire. La corrélation entre le temps de réflexe achilléen et le patellaire est de 0,79. Les temps de réflexe montrent des corrélations élevées avec les mesures physiques considérées comme fonctions de la longueur de l'arc réflexe. Les corrélations trouvées les plus élevées sont celles entre les temps de réflexe et la hauteur. Elles sont de 0,85 pour le réflexe achilléen et de 0,73 pour le patellaire chez le groupe d'étudiants universitaries. Sur la base des résultats du temps de réflexe chez 250 individus, variant en âge de 4 à 35 ans, on ne trouve nulle évidence d'une corrélation entre le temps de réflexe et l'intelligence. Sur la même base, on ne trouve nulle évidence d'une corrélation entre la vitesse de la conduction neurale dans l'arc réflexe et l'intelligence.

Les données sur la base desquelles ce laboratoire a donné auparavant des corrélations se sont montrées inconstantes.

Travis et Young

ÜBER DIE BEZIEHUNGEN VON ELEKTROMYOGRAPHISCH BEMESSENEN REFLEXZEITEN VON PATELLAR-UND ACHILLESREFLEXEN ZU GEWISSEN KÖRPERLICHEN MESSUNGEN UND ZUR INTELLIGENZ

(Referat)

Durch einen Westinghouse tragbaren Oscillographen erhielten wir elektromyographische Bestimmungen von Reflexreaktionslatenzen mit einem Zuverlässigkeitskoeffizienten von etwa .90 für den Achillesreflex und .80 für den Patellarreflex bei einer Gruppe junger Erwachsener beiderlei Geschlechts.

Die Platten zeigten eine Zuverlässigkeit von ungefähr .97 des Achillesreflexes und .94 des Patellarreflexes. Der wahrscheinliche Irrtum der Reflexzeit von einer einzigen Serie von 6 bis 10 Platten ist ungefähr .4 Sigma für den Achilles- und .3 Sigma für den Patellarreflex. Die Korrelation zwischen den Achilles- und den Patellarreflexzeiten ist .79. Die Reflexzeiten zeigen hohe Korrelationen mit körperlichen Messungen die man als Funktionen der Länge des Reflexkreises auffasst. Die höchsten Korrelationen fand man zwischen Reflexzeiten und Höhe. Diese sind .85 bei dem Achillesreflex und .75 bei dem Patellarreflex in einer Gruppe von Studenten der untern Klassen. Auf Grund der Reflexzeitplatten von 250 Vpn. im Alter von 4 bis 35 Jahren, fand man keine Beweise für die Korrelation zwischen Reflexzeit und Intelligenz. Ebenso fand man keine Beweise für die Korrelation zwischen der Schnelligkeit der Nervenleitung in dem Reflexkreis und der Intelligenz.

Die Daten auf Grund derer man Korrelationen zwischen Reflexzeit und Intelligenz in einem früheren Artikel berichtet hatte, haben sich als unzuverlässig erwiesen.

TRAVIS UND YOUNG

THE RELATIONS OF APHASIA*[1]

From the Department of Psychology, University of California at Los Angeles

SHEPHERD IVORY FRANZ

Speech, both receptive and emissive, is a social accomplishment that has attracted attention from many angles. Supposedly the main differential mark of man, distinguishing him from the anthropoid apes as well as the lower brutes, it has been studied by different specialists, but they have almost always been dominated by the concepts of the grammarians. This is to be noticed in the psychological work on verbalization development and on the corresponding developments of writing and of understanding auditory and visual expression. It is also noticeable in the investigations of speech decadencies, such as that in aphasia. From whatever angle the problem of speech is approached, the problem should be recognized to be one. It is a problem of social reactions. The study of the relationships of languages, their coordinations or subordinations, as well as that of the development of language use by an individual, are parts of the general problem. So also are the neurological studies of the relationships of cerebral mechanisms to speech losses and abilities, and equally the studies of language disabilities and of language degenerations in individuals and in groups.

It is with the language of individuals in whom destructions of parts of the brain have resulted in individual speech difficulties that I am here concerned with. This aspect of speech has been the subject of much study since the discovery by Broca of a supposed relationship between destruction of a part of the cerebral cortex and an inability of speech which came to be called motor aphasia. Later, Wernicke described the corresponding converse condition in which a lesion of another part of the cerebrum was associated with inability to understand speech. Subsequently, Marie contended that the so-called motor aphasia is solely a disturbance in the coordinated processes of articulation, whereas aphasia, as such, is a condition in

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[1]Presidential Address, before the Western Psychological Association, July, 1928.

which there is a lessening of intelligence. According to Marie, aphasia is a condition closely resembling the sensory aphasia of Wernicke. More recently studies of aphasics by the English neurologist, Henry Head, have given a newer formulation of the general problem of speech as well of the aphasias. This formulation includes a denial of much that had been widely accepted, as well as a positive statement of the fundamental conditions in speech disturbances. Of greatest psychological interest is Head's conclusion that the aphasias are "disorders of symbolic thinking and expression." Although he discusses the neurological correlates of speech disturbances, more striking is his insistence on better methods of examination and analysis of cases, and his further insistence on the psychological interpretation of speech defects.

This, as Head points out, is in line with the earlier explanations of Hughlings Jackson, who failed to get the anatomically-minded neurological world to agree with him, that the aphasic was primarily defective in the "formation of propositions." Head uses the term "symbolic thinking and expression," by which he means the disturbance of "complete and highly developed" speech function. It is not the elemental processes which are affected, but the elaborated processes. Although the aphasic disturbances are similar in a general way, Head thinks they are also sufficiently different to warrant clinical classification into four forms: verbal, nominal, syntactic, and semantic. In these forms ideas or images are not necessarily destroyed, but in many cases "they cannot be evoked with certainty at will or at command." At the same time he says that "behavior is affected in a specific manner; an action can be carried out in one way, but not in another." Again, "sense impressions and images can be appreciated and recalled, but the aphasic may have difficulty in expressing the relation between them." He is willing to admit that the four forms into which he divides his cases are not fixed and that there is overlapping or indefiniteness.

I have been able to classify only superficially the aphasics with whom I have worked in accordance with the old rubrics of motor and sensory, only after death as cortical, subcortical, or transcortical, and only in a general way into the divisions of Head. The disturbances appear to me to be more general, as Head admits. Aphasics show notable variations in the speech ability at moments of re-education which do not allow us to fit them into the current classifications. Perhaps because of this we may need to change the

classification, or give a formulation in somewhat different terms. In the cases I have to cite, examinations show the patients to be intermediate between the relatively separated groups reported by Head, but with tendencies more towards one or two of his clinically defined forms. It is, however, some of the general phenomena which constitute this report.

An intelligent street-car motorman going to work early in the morning was attacked by a footpad, fell to the sidewalk, and at the hospital was found to have a fractured skull. He was unconscious for several days. Operation relieved the intracranial pressure, but speech was completely lost. At the end of several months the aphasia was evidenced largely by what might be called a mixed condition, showing symptoms mainly of the "nominal" and "verbal" types of Head, although the losses were such that he might also be considered to have "syntactic" and "semantic" defects. He could not read aloud but he could gather the sense of printed matter in a newspaper if he was interested in it (or if something in a headline or in the body of an article attracted his attention). He talked rather freely although somewhat incoherently because he had difficulty in finding words to express some of his ideas. When asked to name articles of clothing and objects in common use, his performance was irregular. Some were immediately and correctly named, others could not be named in a period of several minutes. Some were named quickly and accurately on one day, but on a succeeding day they were not named or they were inaccurately named. He appeared to take in the points of jokes which were told to him; he found his way about the city alone; he was able to ask simple questions and sometimes to understand statements regarding times, places, people, and things. His behavior was, in general, excepting speech, obviously normal.

For us at the present moment, when he is recovering a fluency and intelligibility which approaches simple normal speech, interest centers in his difficulty in verbalization regarding things not common in his daily life. A cardboard square he named correctly but after a noticeable pause; he called a circle "one line." Shown a cardboard oval and asked to name it, his speech solution is characteristic of his ability as well as of his disability. He replied: "I know that, but I can't just say it. It is not like the other things, square, and so on. I know how to do it. You go to the store and buy something. The man puts something around what you buy" (show-

ing by movements the process of wrapping a bundle). "Then he puts another thing around it" (showing at this time the process of tying the wrapped bundle with twine). "Then you take it home. When you get it home you take off what you got on the thing you bought in the store" (illustrating the untying of the twine). "Then you take two things I should have here (fumbling under the left lapel of his coat for pins which he apparently usually carried in that place) and you put them in this way" (showing the positions of two pins about 3 inches apart). "You then put the thing you got from the store on the two, and with a pencil, you make the lines."

To one unacquainted with the everyday method of drawing an oval, the description may appear unintelligible, but the processes (except that of making a purchase at a shop) are exactly described, although the words oval, twine, and pins are not used. It will be noted, however, that equally concrete situations are accurately worded: square, man, store, home, pencil, and lines. At the same time, there are other symbolic expressions of equal importance: "buy," "puts around," "take home," "take off," "put in," "two," "make." In other instances this patient described objects by their uses, as, for example, when shown a picture of a box of strawberries, he replied: "I like them; I ought to be able to say it. My wife bought some boxes yesterday, and she was making them up into jam, I know, but it has just escaped me." Then, after a pause, "Strawberries."

In these two examples, and many more could be cited for this patient and for several others, the attempt at a verbal solution is apparent in the process of association. Describing the thing, noting something about its use, how it is made, etc., are methods which all of us follow who have not had recognizable brain injuries, especially with items for which descriptive words are difficult. Take note, for example, of the ways in which the theory of relativity is explained by those relatively unskilled in physics and in mathematics. Cross-word puzzles are not supposedly psychological problems, but the attempt at a few solutions will reveal many speech difficulties which are analogous to those which are reported to be characteristic in some aphasias. Difficulties in getting away from a key word, thinking about and around a misconception which later may be recognized to be a misconception, the attempts to arrive at solutions by thinking and talking about the ideas suggested in the problem, are instances

which closely resemble the attempts of the aphasic to overcome an inability in the use of terms which are appropriate to the occasion. For the aphasic, the occasion may be more complex than a crossword puzzle. And perhaps we may add, as a further analogy to the aphasic difficulty, that exhibited by many people under other conditions in which set responses are demanded. The student's reply to a question, "I don't know," is not infrequently an inability to give voice to ideas which he may have but which he cannot at that time verbalize in exact terms. Frequently also the student's tendency "to beat around the bush" is only an attempt at an associative linkage of words (or ideas) which he has difficulty in getting directly and immediately. And the condition of stage fright does not materially differ from some of the difficulties encountered by those who have had cerebral injuries whom we call aphasics. Stage fright is mostly an associative (mainly verbal) difficulty, which can well be described as a disturbance of symbolic thinking and expression. These examples in so-called normal people are instructive analogies of difficulties in symbolic thinking or expression which we tend to refer exclusively to the aphasic. If we wish to add one more example, we may consider the verbal and the nominal defects in an individual who is acquiring a foreign language. When a few words have been acquired they may be uncertainly used, and there exist syntactical difficulties. Much later and only after much study and experience with both expression and impression does the learner acquire the ability to comprehend the full meaning of "jokes set out in print or pictures." Until that time he has not overcome the semantic difficulty.

Another illustrative condition was found in an aphasic patient who was a mixture of the verbal and nominal types. The man was an architect who had been hit on the head by a brick which had fallen from the tenth story of a building the construction of which he was supervising. For a relatively long time he could use only two affirmative, one negative, and two affective expressions. Later, he recovered his ability to count, and to name in sequence the letters of the alphabet. He looked over the daily paper, and would sometimes call attention to the mention of friends or familiar scenes, but in this he was not constant. He played dominoes, he could count and he could also cheat. He did not apparently take in printed jokes, but he could obviously understand those presented vocally. A paralysis of the right arm prevented writing or drawing, but he attempted to

write with his left hand and also to draw. His (left hand) signature was at times very badly done not only as a motor accomplishment but in terms of perceptive satisfaction. He understood the values of moneys, and numerical values relating to his occupation were appreciated to a considerable extent, although in speech the numbers might be wrongly given by him. In the course of re-education, he acquired certain abilities which were not apparent in the early months following the accident, but after two years he had not learned to vocalize from printed material. His vocabulary at that time was restricted to very few expressions, and, under the older neurological classification, he would have been classed as a complete motor aphasic.

Because of his apparent interest in the daily newspaper and in professional journals, I thought it would be well to determine how rapidly he could form visual-verbalization responses which we call "reading aloud." He had not been able to give the appropriate verbalizations to any printed or written material I presented to him, although in other ways he showed he was able to take in some ideas, as, for example, when he called attention to the names of familiar places and persons which he saw in print. In this experiment, I began with the word *cat,* printed on a card, which I exhibited visually and pronounced. He repeated the word aloud about 30 to 40 times, after which I put the card on one side out of sight. Next I showed him the printed word *hen* and had him repeat it after me. And in the case of the word *cat* he kept repeating it, as if to impress it upon his memory, after which the card was also put on one side. The card with the word *cat* was then shown to him, and he immediately responded "mice." When told "no" he said "dog" and when told "no" for the second time, he said it was "rat." For several minutes he struggled with the word, many times giving responses, all of which were incorrect. Some he recognized to be incorrect, and he then attempted to correct himself with another verbalization. The word *hen* was then shown to him, and his first reaction was "egg." Subsequently he gave in successive attempts when I refused to accept his responses, the words "chicken," "duck," and "turkey." In none of the trials with the two words did he exhibit the ability to give the appropriate responses "cat" and "hen." It is important to note that none of the words used as reactions to the two visual stimuli (the printed words *cat* and *hen*) was a part of his usual vocabulary and when the corresponding printed words were shown to him he was

unable to vocalize them. These facts were taken as a suggestion for a change in the plan of procedure.

Since he was able to utilize words which were associative reactions, I said to him, "man and—woman," then "night and—day." The giving of these two associative groups was apparently sufficiently attention-attractive, for when thereafter I said, "shoe and—," he immediately replied, "stocking." This experiment was continued with many words over a period of 40 days, on 15 of which repetition tests were carried out. In all there were 687 tests, with 242 stimulus words. In 564 tests there were associative verbal reactions, and in 123 there were failures of definite verbal reactions. There were 292 individual words used by the subject, which indicated definitely a vocabulary use far beyond that which many other tests and observations had indicated in daily work over a period of several months. In the individual tests there were sometimes more than one response word given, there being a total of 614 responses in 564 tests. To only 16 of the 242 stimulus words were there complete failures in response. Some of these stimulus words to which he failed to respond are: *have, heart, leaf, with.*

In some cases the responses were apparently fixed: five times each for *up-down; east-west; cup-saucer; chair-table; collar-necktie; north-south.* On the other hand, some reactions were individual, but there was an associative correlation even though the reported association appeared to be individual (in the sense of Kent and Rosanoff). As examples, to the stimulus word *light,* the subject gave respectively in different tests: *air, dark,* and *cold;* to *land* he responded at different times, *water* and *ocean;* to the stimulus word *pen* he gave, in individual tests, the reactions *table, chair, papers, ink,* and *writing.*

In most instances the reported associations were easily understandable in terms of common use, but at times they were apparently "mediate" in character. Why the subject gave *necktie* as a response to the stimulus word *powder* may be readily guessed, but it is more difficult to guess why he responded with the words *salt* and *vinegar* to the stimulus word *whiskey.* The stimulus word *wood* brought forth in different tests the following responses: *vim, window, chicken, hen, turkey.*

In only four tests were meaningless words (i.e., words not immediately understood by the experimenter) reported. These were *pudden, mimi, sol,* and *chiff,* which were responses respectively to *push, aunt, lost, hand.* None of these neologisms is far from a word

which might have been reported, and the recorded meaningless words may be quite meaningful. The test was not interrupted in order to question the subject at the time of the supposed meaningless responses, and later he had forgotten the reported words. I can see, however, that push-*pudden* may have been a poor vocalization (motor defect) for *push-pushing;* aunt-*mimi* for Aunt *Mamie;* lost-*sol* for lost-*soul;* and hand-*chiff* for *handkerchief.* As experimenters or observers, we have the tendency to demand reactions which are readily and immediately understandable to us, but from these examples it is just as reasonable to suppose that we often fail to take in some of the so-called speech situations. We know that in many instances the deaf person who has learned to talk vocalizes in a way which is difficult to understand, and we also know that children carry over to other environments many expressions which are understandable to their immediate social circle. The reactions of the deaf and those of the child may be "adequate" speech reactions in some situations but "inadequate" in others. So also the language of the Hottentot in his own village and in New York.

From the grammarian's standpoint, it is of interest to note that in the association responses this aphasic subject was not limited to the use of substantives concerned with concrete things, but that he also used abstract words, verbs, adjectives, and adverbs. Verbs as stimuli often called forth verbs, adjectives and adverbs often called forth corresponding words, and the concrete nouns sometimes were responded to with abstract ones.

In a number of experiments when the subject reported associations which appeared to me to be "mediate," I then suggested reactions which seemed more "common," to see whether or not the subject would adopt my responses. In most cases the adoption was made, but in some instances he appeared perversely unimproved. The stimulus word *blind* brought forth the reaction *good.* Although I suggested *deaf* as the more immediate association, in the successive experiments, the subject gave, respectively, *soft, wet, stupid, sober, new, old,* and disregarded my excellent suggestion. To the stimulus word *black* he gave in successive tests *table, chair, yellow,* after which he adopted *white* as I suggested, but in the next test he responded with the word *small.* The word *cellar* gave rise to *sweet, winter, summer, April,* and *sweet,* and only on the sixth test did he adopt my suggestion *garret.*

In this case, therefore, although the patient appeared to be limited

in his use of words to two emotional expressions and to several denoting affirmation or negation, it became apparent that his capability in word responses was fairly good, and that he had a vocabulary which could be brought into play provided the appropriate stimuli were given.

His other symbolic reactions, using Head's terminology, were also complex and they exhibited conditions which are of interest. Head has called attention to difficulties of aphasics in drawing pictures of an elephant and in making a ground plan of a room in which they might be. This patient, when first asked to draw a ground plan of the room in which he was working, failed to show any way of ingress or exit, but when I called his attention to the defect which no good architect would permit in a plan, ever thereafter he did not fail to show all of the important points. Moreover, a few days later, when asked to draw plans for a house containing four bedrooms and two baths on the second floor, a large sitting-room, a dining-room, kitchen, pantry, den, and accessories on the first floor, he made understandable sketches of ground plans with these requirements, using his left hand (the right side being paralyzed). He drew the two floor plans on a sheet of paper, the two floors fitting together very well. Then I asked him to draw the front elevation on the same sheet of paper. In order to do this he had to use the upper portion of the paper and he was compelled to turn it through an angle of 90 degrees. In that position the floor plans nearest him showed the right side of the house, but he had no difficulty in mentally turning the two plans into a front elevation. His symbolic thinking in this respect was far superior to that of many normal individuals. In later sketches which he made and to which I objected because of supposed errors and inconsistencies, he was able to show in his drawings that I did not take in the full significance of the situations. In one instance, for example, in sketches in which there was a question of a stairway, he was able to show me that my symbolic thinking in architectural terms was not as good as his, although he had lost part of his brain.

The use of the alphabet in attempting to read and the use of number series in attempts at counting, which many aphasics exhibit, are samples of attempts at associative reactions. They differ from those of normal speech ability in no essential manner. The aphasic's use of one reacting word to many situations or stimuli is not unknown among normal people. The child may use the word *ran* (for orange) to indicate that he wants something to eat or to drink or that he

wants something other than what his mother or his nurse gives him. It may also be used as an expression of pleasure. The frequent and repeated use of "thing" by many pupils in describing many elements in a relatively unfamiliar situation is a comparable example. The aphasic's use of words in some combinations and not in others is also a point of interest. He may not be able to say *ink* when a bottle of that fluid is shown, but after some hesitation as if to fix the right word he may reply with apparent disgust, "That is what you get in a bottle of ink." The aphasic fluctuates greatly in his responses. This appears distinctly in the process of re-education, and it resembles the difficulties in speech which a person with uninjured brain may show in his efforts at learning a foreign language.

In the aphasic there is a form of dissociation. This dissociation is observable for some processes at one time and not at another. It is not a dissociation always of ideas, it is a dissociation of ideas and reactions. It is a dissociation of ideas and some kinds of reaction (maybe verbalization) with the retention of other kinds of reaction (as in writing). The dissociations in the clinical varieties are not mutually exclusive; they are overlapping. It is this concept of speech, in aphasics and others, which I think is important to recognize at this time. For clinical purposes some kind of classification may be demanded. But the clinical classifications are not necessarily explanatory. The aphasic has lost some of his associative ability, and this is evidenced in the terms of the grammarian in his inabilities with substantives and their connections, in difficulties of expression, vocal and manual, and in difficulties of fitting together perceptive states and reactions. In a group of patients there may be obviously similar speech disturbances, but probably only because most of us have learned by education to associate in similar ways. It must be understood, however, that the losses are symbolic only in so far as practically all of our activities are symbolic, even such a commonplace act as putting on one's shoes. They are symbolic because we have developed activities or associative reactions of a social character.

In the aphasic, it is the associative losses which are important to discover. And it is the relearning, reacquisition, or re-formation, of associations to which we need to attend. These are, I think, the *general* manifestations of aphasia. They are also the underlying conditions in other speech disturbances in individuals who have not had brain destructions. Brain destruction may be an important incident in making speech associations difficult, but at the present time

the brain destruction is neither explanatory of the associative losses nor can it be definitely classificatory until we further investigate speech from the standpoint of abilities and disabilities of association.

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LES RAPPORTS DE L'APHASIE

(Résumé)

Les classifications cliniques des aphasies ne sont pas satisfaisantes, ni comme description ni comme explication. A plusieurs égards les difficultés soi-disant caractéristiques dans l'aphasie sont pareilles à celles chez les normaux. On cite des résultats expérimentaux avec des aphasiques lesquels montrent dans des cas individuels de si grandes différences de verbalisation et de telles capacités dans d'autres réponses de parole que la classification clinique d'un malade ne peut rester constante. Les expériences montrent d'ailleurs qu'on peut résumer les dérangements sous la vue générale de "difficultés dans l'habileté d'association, ou dans la combinaison, des idées et des réactions." Quelques-unes des conditions de l'habileté, et ainsi de celles de l'inhabileté, à associer paraissent dépendre de la présentation de stimuli appropriés.

FRANZ

ÜBER DIE BEZIEHUNGEN DER APHASIE

(Referat)

Die klinische Klassifizierung der Aphasie ist nicht befriedigend, weder als Beschreibung noch als Erklärung. Die vermeintlich charakteristischen Schwierigkeiten bei Aphasie erscheinen manchmal auch bei normalen Menschen. Man führt experimentelle Ergebnisse mit Aphasie an, welche in individuellen Fällen solch, verschiedenartige Schwankungen in der Verbalisierung und solche Fähigkeiten in andern Sprachreaktionen aufweisen, dass die Klassifizierung eines Patienten wohl nicht immer konstant bleiben dürfte. Die Experimente zeigen ferner, dass die Störungen unter dem allgemeinen Begriff, Schwierigkeit der Assoziationsfähigkeit oder der Kombination von Ideen und Reaktionen, zusammengefasst werden können. Manche Bedingungen der Fähigkeit, und umgekehrt der Unfähigkeit der assoziativen Verbindung scheinen von der Anwendung wirkungsvoller Reizmittel abzuhängen.

FRANZ

OCULAR DOMINANCE IN HUMAN ADULTS*[1]

From the Psychological Laboratories of Stanford University

WALTER R. MILES

THE QUESTION OF EYE DOMINANCE

It requires but a moment's observation of the ordinary strabismic person to tell with which eye he fixates, and when once this is determined we "look him in the eye." For him the views of the two eyes are so different that both cannot be used or attended to at the same time. The better eye has won out in the competition for supremacy. It may be better because of greater acuity, more brilliant image, because less muscle strain is required for fixation, or from a combination of these and other factors. The strabismic comes to use this superior eye habitually, and the view of the deviating one is neglected much as after-images and double-images are disregarded by people of normal binocular vision.

In persons of normal vision in whom there is no marked lack of muscle balance we may or may not observe from the eyes, face, or behavior some indication of ocular dominance. Should there be evident opacity of one lens it may be assumed with fair certainty that the opaque eye is the subordinate one. If the face is structurally asymmetrical the dominant eye will probably be on the shorter or more contracted side. If the individual can wink one eye and not the other the winking eye will probably prove to be the subordinate one. The person who has a dominant eye by reason of very unequal vision may show this fact in certain tricks of behavior and especially by the head posture when giving careful visual attention. It is a familiar fact, however, that many people never discover their own eye dominance, and their most intimate friends are equally ignorant of the condition even when partial blindness exists on one side.

In marked strabismic cases the predominant use of one eye is about as evident as right- or left-handedness. Nearly all other individuals, however, must be placed in some test situation if the dominance is

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to be made evident. As a rule, the tests generally employed have been so largely subjective that results obtained by them were inconclusive, and the examiner was left in doubt of their accuracy.

Reasoning by analogy from handedness, eyedness has been assumed to be general. But as a matter of fact the functioning of the two motor coordination systems (hand and eye) are really not analogous. Picking up a needle requires just one hand and to use both for such a task would be very clumsy. The needle is, however, viewed with both eyes as you reach for it and, typically, is threaded on the basis of binocular impressions. Consider another example, throwing is done with one hand, but both eyes remain open. In shooting, however, we *learn* to close one eye for here the task has solely to do with aiming not with gauging the amount of force to be released. This monocular performance is a learned reaction, as is evidenced by the fact that children begin shooting with both eyes open.

For the eyes there is no highly developed or immediate realization of right-side experience in contrast with left-side (11, p. 403). If a person with normal eyes looks at the landscape through his spread fingers held in the direct line of vision so that parts of the field are obscured, he has no clear sense of which parts of the view are seen with the right eye and which with the left. He discovers this only by closing or covering the eyes one after the other.

It is typical, then, to use one hand or the other and very habitually one, but in vision we characteristically use both eyes and just occasionally only one in some special circumstance or as an aid to some particular motor skill. In the light of ordinary human behavior such as we have just discussed, how are we to understand the statements of Gould (2), Southall (3, footnote, p. 7), Parson (12), Shastid (15), Sheard (16), Miles (5), and others that a dominant or leading eye is found in practically all cases examined, whether men, women, or children? Probably all we can say definitely at present is that nearly everyone can be forced into a choice and that, in general, the choices of a single individual will be consistent. But since there has been no generally accepted objective test by which reliable data could be gathered over a period of time, we cannot pretend to know the real significance of eye dominance and the relationship it may have to physical development, efficiency, and general behavior.

The specialist examining and prescribing for eyes doubtless should

know which eye is the team leader and, if possible, the degree of functional dominance. Aside from one or two obvious uses of this information, there arises the question what to do about it. Is dominance a thing to be encouraged or held back? Should the patient know of his own tendencies in this respect? Does the presence of a dominant eye actually work toward a physical or functional distortion of the face? Is dominance a favoring condition for the development of motor skill, fine visual discrimination, and high achievement in general? At what time in the individual's life does the preference begin and can it be ascribed to adaptation in an environment where minute visual detail is stressed? How readily and with what result can dominance be reversed in case of need? The questions are many, and the problem is perhaps one of much importance to each individual. Answers will have to wait on data. The first step is to have some means for outward, objective measurement of the hypothetical tendency. If this phenomenon of behavior is real and general, its bearings on development and function will reveal themselves if we take ocular dominance into account as we are studying visual performance and motor coordination. It would seem that the testing should be the simplest that is conformable with due care in so subjective a matter and because of a possible unfavorable effect on the patient he should not automatically become aware of the results.

Various Tests For Dominance

The ways of testing for dominant eye may be grouped under five heads, but it must be understood that these arbitrary classes are not mutually exclusive. They are designated by terms which refer primarily to the manner of conducting the test.

1) *Comparative Monocular Brilliance*

a) *Consecutive monocular viewing of the same field to note if the brillance is alike for the two eyes.* The two eyes must be open and closed for periods of equal length. This is too subjective a test to be of much use with untrained observers.

b) *Retinal rivalry with the stereoscope.* This is simultaneous viewing of different fields by the two eyes. The person tested must decide, and he alone can tell, if the two fields blend or if one usually predominates. The stereoscopic slide should be turned end for end to discover if this shift changes the appearance of the pic-

ture. It will change it if the slide is well arranged and if there is dominance sufficient to be demonstrated in this manner. The difficulty with the stereoscopic test is that attention is usually directed to some specific detail in one field or the other, and so a fluctuating rivalry is present as attention shifts, and results for dominance usually seem uncertain.

c) *Pascal's (12) chromatic brilliance test.* The patient, with eyes corrected to equal acuity and with both eyes open, looks at a white light. A red disc (glass) from the regular trial case is placed first in front of one eye and then in front of the other. The patient is asked to tell with which eye the light appears more red and about how much more. The red glass is shifted rapidly back and forth for a check-up. The result obviously depends too much on the patient's verbal description.

d) *Double image test.* With his back to a window and the visual field in good light, the patient fixates a dot on an evenly illuminated and unfigured wall several feet away. Between his face and the dot he holds a pencil vertically at arm's length. Two images of the pencil will appear, that from the right eye will be on the left side. He is to say which of the two images is the more vivid, dense, or sharp in outline. The more vivid or colorful image is from the dominant eye. The double image test serves satisfactorily with many people and they promptly report a difference. Although the method is highly subjective, it is useful if the response is prompt and in definite terms. Many can see no difference, and some fail to see the pencil images.

e) *Comparison of monocular after-images from the same stimulus.* Each eye is worked with separately. The after-image is from an exposure of a given duration and projected on a certain background at a given distance. The attempt is to compare the after effects of equal but non-synchronous exposures. The eye that gives the stronger or more persistent after-image is assumed to be the more sensitive and to be the dominant eye.

2) *Comparative Monocular Acuity.* When a person designates his "better eye," he usually means the one with which he can most sharply see fine detail. The eye that has the better acuity is probably most often the dominant eye, although acuity does not seem to be the determining factor, and cannot be relied upon to indicate with certainty which eye is habitually dominant.

3) *Comparative Fixedness of the Visual Image When the Eyes Are Alternately Covered.* Orthophoria, or ideal muscular equilibrium, is said to exist when neither eye shows any movement on shifting from binocular to monocular vision. Duane (1) has described this method of studying latent deviations and calls it "parallax test."[2] If movement is shown, that is, if there is some heterophoria, then the eye that moves the lesser amount is supposed to be dominant. When the examiner can see no movement it is still often possible for the examinee to note some lateral shift of the image when the eyes are alternately covered. Such shifting of the image has to do with eye dominance, but it is difficult to interpret the results with certainty.

4) *Comparative Persistency in Convergence.* Mills (9) has described a convergence test which he urges as an indicator for eye dominance. Using a mirror as a target he moves this slowly toward the subject who is asked to fixate the center. After the near point is reached and passed, the subordinate eye tends to diverge. The eye that continues the attempt to converge is held to be the dominant one. The test is objective but causes the subject quite a little discomfort if repeated, and without this the results are uncertain.

5) *Unilateral Sighting*

a) *Aiming a gun.* This is probably more a test of handedness than of eyedness. Right-handed people hold the piece most conveniently with the butt pressed against the right shoulder, supporting it and pulling it firmly against the body with the left hand. The right hand gives some support and firmness to the hold but is largely free for the more delicate trigger work. With the gun against the right shoulder it is evident that the right eye is the natural one to be used. The situation is thus prejudiced by the hold that is taken of the gun.[3] Pistol shooting perhaps more characteris-

[2]Maddox (4) states that Alfred Graefe was the first to use the sudden screening of the fixing eye as a means of producing a fixating reaction in the other eye to show if deviation is present. Ordinarily a candle or a regular electric lamp has been used as the target for such tests. A still better object for fixation is the electrode of a Neon Glow Lamp [see Miles (8)]. Operating on alternating current the Neon lamp gives flashes of light which become apparent only when the eye moves.

[3]Cases of left-eye aiming while the gun is held in the typical right-hand manner are occasionally found.

tically brings out eye dominance but even this can hardly be trusted as a test.

b) *The Porta-Rosenbach alignment test.*[4] In the test most commonly used in eye examination the seated patient is given a pencil with the instruction "line up this pencil with the dot on the wall" or "sight past the pencil to the dot." It is really a matter of aligning three points, the mark on the wall the pencil held as a vertical, and the patient's eye—but the question is which eye? If the examiner stands directly behind or a little at one side and in front he can tell which eye is used as the third point although the patient may not be aware that he is using only one eye. This test situation actually results in a double image either for the pencil or for the mark on the wall, or for both if a position intermediate between the pencil and the wall is fixated. It is rare that a patient asks, "With which eye do you wish me to line up?" Usually, accustomed as he is to disregard the second image, he seems to see but one image apiece for both mark and pencil. If twin images of the pencil are seen one of them naturally lines up first, thus usually indicating the preferred eye. The alignment test, while doubtless a very serviceable indicator for eye preference, is to be criticized for the manner in which it is usually conducted. The patient as a rule takes the pencil in his right hand and brings it in front of his face from the right side while looking at the distant mark. The pencil thus comes in line with the right eye first. It is a question whether this right-handed manipulation of the pencil does not prejudice the result. The precaution should probably be taken of having the patient clasp his hands in his lap holding the pencil between the two thumbs and then bringing it more or less vertically up before the face. Refinements of the test consist in using a headrest, peep-holes for the eyes, and a pendulum or other form of movable object to be brought into alignment by using both hands at once.

c) *The peep-sight test.* If a person wants to peep through a knot hole or look through a small telescope, he has to express preference by bringing his head into the position for one eye or the other. When the aperture to be looked through is fixed and needs no holding or adjusting by the hands, when there is no obstruc-

[4]It is not clear who first devised the alignment test. For some notes on the subject see Miles (5).

tion which would interfere with the position of the nose and so force a preference, and when the observer does not realize that he is being asked to show "which eye," this test is reliable.

d) Parson's (12, 13, 17) manuscope (or manoptoscope) test. Parson, in connection with a discussion of left-handedness, described and recommended an apparatus which he calls a manuscope. Essentially this test consists of looking through a funnel-shaped hood held with the large end over the face. The device is about 9 inches long. The narrower opening is round and 1 1/4 inches in diameter. The wider end fits about the forehead excluding light in much the same way as a stereoscope hood.[5] With this cone held in position to the face, if one placed a pencil at the center of the small opening, he could see it clearly binocularly; the eyes would be rather strongly converged. But if the pencil is moved off several feet, both eyes cannot see it at the same time. If, for example, the right eye is looking at the distant pencil, the left inside area of the cone hides the pencil from the left eye. The arrangement can best be understood from the simple diagram Figure 1. *R* can look through *X* and see *O* in the field *MN*. *L*, maintaining its coordinate adjustment with *R*, is properly directed for looking at *O* (provided there is no heterophoria) but cannot see it, due to the side of the funnel at *S*. If the side were cut away, both eyes could see *O* at the same time. But, as it is, we have a field *PQ* that is exposed to the left eye through the opening *X* but, due to the fact that both eyes are directed at *O*, the image for the *PQ* field falls considerably to one side of the fovea in *L* and hence may be easily neglected or suppressed. The result is that the patient sees clearly only the field *MN* and, although both eyes are open, he is really using only his right eye—the dominant eye. Parson has arranged a triple-target for carrying out the test. This target we have represented as *YTZ* in our diagram. The patient is told to bring the manuscope up to the face and look at the central item *T*. While viewing this he is asked whether he sees anything else. If he reports seeing *Z* off at one side then the examiner knows that he has directed his right eye at the central item as in the diagram and that he is probably right-eyed. If he reports *Y* this indicates the left eye as

[5]This hood for viewing is about like that employed by Hering in his investigation of binocular perspective, see Tigerstedt's *Handbuch der physiologischen Methodik*, Leipzig, 1914, Band III, p. 160, Figure 29.

dominant. If the examiner is in doubt, Parson's instrument is so arranged that a curtain can be dropped in front of either eye.

Parson's ingenious test forces a choice and, although it is quite simple, it has little resemblance to the familiar sighting or gun-aiming in which the examinee may have had some experience.

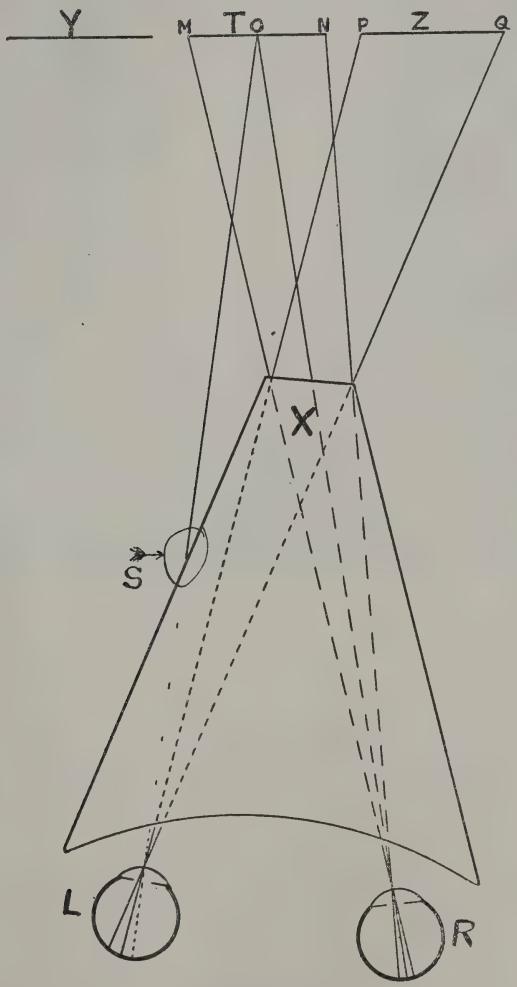

FIGURE 1

RELATION OF THE *V*-SCOPE TO THE LINES OF VISION FOR THE TWO EYES WHEN THE RIGHT EYE CAN SEE ONLY THE TARGET *O*

The unilateral sighting tests, of course, involve brilliance, acuity, and muscle balance. They represent a functional parallelogram of factors and seem to offer the most trustworthy results. Of these tests, Parson's promises most but is to be criticized for the way in which it is conducted. (*a*) By asking the patient "what else he sees," attention is directed to the fact that there are two fields of view. He may not know which belongs to which eye (by winking he can easily discover), but he is puzzled and so further trials with him are more or less spoiled. (*b*) The test should be so carried out that the hood is brought up to the face by both hands in a symmetrical manner, not favoring either eye as one-handed manipulation is likely to do.

The A-B-C Vision Test

The test by which the data presented in this paper have been obtained is a modification of the Parson method. The designation *A-B-C* has been chosen for two reasons. The writer believes that in examining a pair of eyes the first thing that should be done, the alpha of the examination, so to speak, should be to determine whether there is a dominant eye and, if so, which it is. In the second place, the patient should not, at this stage, know just what is being determined. It is inappropriate to call it "Dominant Eye Test." The *raison d'être* given the patient must be something quite different so that he will respond naturally and the examiner may have adequate opportunity to study his behavior. *A-B-C* stands for Area-Blackness-Comparison Test. The patient looking through the cone, here called *V*-Scope, for a short interval sees a card held by the examiner. The card has on it two discs which differ in area and blackness. After the card has disappeared, the patient tells which disc was larger and which was the darker. Nothing whatsoever is said about right or left eye or "What else do you see?" for the examiner, just like the reader looking at Figure 3, directly observes which eye is used.

The *A-B-C* Vision Test and the extremely simple scheme for carrying it out can be readily understood from Figures 2 and 3. The secret of the test is in having some specific test object (or set of these) toward which the attention and interest of the patient are directed. With this element included, the test may be repeated and repeated without the tested individual "catching on." The

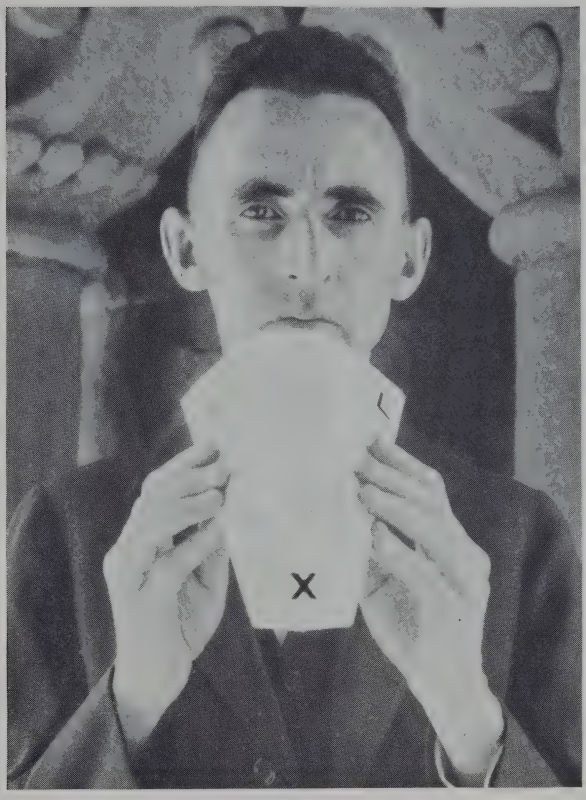

FIGURE 2

SUBJECT HOLDING THE *V*-SCOPE JUST PRELIMINARY TO BEING TESTED FOR EYE DOMINANCE

writer has tested, among others, several instructors in physiological optics, ophthalmologists, optometrists, and the like, and even they have failed to realize that they were exhibiting their ocular dominance.[6]

RESULTS FROM TESTING ADULTS

Through the use of the test for ocular dominance outlined above, data have been collected at Stanford on about 600 individuals of

[6]For detailed instructions see the former article by Miles (5). The testing equipment which consists of three *V*-Scopes, a set of specially printed spot cards and instructions can be secured from the author.

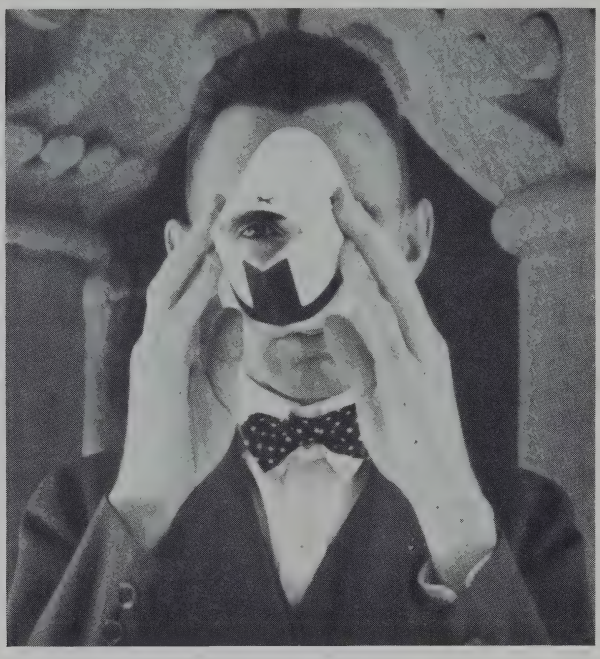

FIGURE 3

LOOKING THROUGH THE *V*-SCOPE AT A TEST CARD HELD BY THE EXAMINER
There is no mistaking the fact that the patient is using his right eye, but he has the impression that he is looking at the test card with both his eyes.

whom two-thirds were men and one-third women. In every instance the examination consisted of five trials or more. By a "trial" is meant the looking at one card through a *V*-Scope thus giving the examiner an opportunity to observe directly which eye was used in that particular instance. Ordinarily the five trials were made in succession. Subjects did not use the same *V*-Scope in two consecutive trials, and so a new adjustment of hands and arms was necessary for each attempt. If the results were not consistent for five trials, that is, if the same eye was not always used, the first set of five trials was followed by a second set. For our present analysis we may consider the data on eye dominance in certain subgroups.

Special University Group.[7] Forty-five individuals, university students (psychology majors) and university professors, all of them interested in visual problems, were tested in the regular manner. Each test consisted of five trials with the *V*-Scope, followed by a second five trials after an interval of 15 to 30 minutes during which the individual was occupied with other tests.

Of the 45 persons, 41 gave perfectly concordant results, with all 10 trials showing either left-eye dominance or else right-eye dominance. The 4 cases that gave mixed results were all men: Subject A gave 1 *L*, followed by 9 *R*; B gave 1 *R*, followed by 9 *L*; in both instances the first trial was atypical and, if the test had been conducted according to Parson's procedure, erroneous conclusions would have been drawn. C in his first 5 gave *L R L R L*, but the second 5 were all *R*. In his personal report, this man stated that he was a mechanic, 27 years old, and that he believed both eyes to be equal. He is left-handed but actually demonstrated equal speed in the coordination of both hands. D gave first 5 *L* and then later 5 *R*. This man was right-handed and in his report stated that his left eye was the better one. A and B have a dominance habit but C and D are doubtful.

Of the 43 members of the group who showed eye dominance, 29 (67%) were right-eyed, and 14 (33%) were left-eyed. We may compare with these figures the results for handedness. Forty-one members of the group were right-handed, and of these[8] 28 were *R*-eyed and 12 *L*-eyed and 1, Subject D, as stated, gave results 5 *L*, 5 *R*. There were 4 cases of left-handedness, 2 of these were *L*-eyed (one had a paralyzed right hand so he was forced to use the left), one was *R*-eyed and one (Subject C) gave mixed results, 3 *R* and 7 *L*. No significant difference appeared between the performances of the women as compared with the men. Some of the results of Woolley (17) seemed to confirm data of Karl Pearson that the men favored the right eye and the women the left. At present we

[7]Most of these data were secured during the summer of 1926, in collaboration with Mr. Paul Crabb, a graduate student in psychology.

[8]We can state these results with some confidence. They are not based simply on the subject's own appraisal nor on the observation of him when writing, but are all derived from elaborate series of tests in each of which he had to show his natural hand preference without knowing that this was the purpose of the test. In certain of the tests the relative skill of the two hands was measured.

have no clear indications by the *V*-Scope of there being such a tendency.

High School Group.[9] These results are on 50 high school students tested at the Palo Alto High School; the group, composed of 26 boys and 24 girls, was tested solely for eye dominance. Each subject was carefully questioned about faults of vision or difficulties with the eyes, speech troubles, and handedness of themselves and their parents. The individuals were all well known to the person who examined them. Among the 50 there were 8, 5 boys and 3 girls, who gave one or more inconsistent trials. Two of the 8 were left-handed. In no case was the ocular dominance habit in doubt as judged by a second set of 5 trials. For the total group there were 32 (64%) that gave right-eye dominance, and 18 (36%) who showed left-eye preference.

A special effort was made by the examiner to secure left-handed subjects and 6 were found, 2 boys and 4 girls. Three of the 6 gave *R*-eye and 3 gave *L*-eye dominance. In only one case was there history of difficulty with the eyes and this case showed *L*-eye results. There were 6 other high school students who, from their answers to various questions, showed fairly clear evidence of having been left-handed when they entered school. Three of these showed *L*-eye and 3 *R*-eye preference. For the clearly right-handed individuals, 38 in number, the results show 26 *R*-eyed and 12 *L*-eyed. All in all these data give us about the same impression as those for the more mature subjects. The chief difference is in the larger number of cases of inconsistency among the high school students. Of 19 who stated definitely which was their better or aiming eye only 11 had their statements confirmed by the test.

Grade School Group.[10] These children ranged from 9 to 14 years. There were 17 girls and 21 boys. Only 4 cases, all boys, gave mixed results, but even these show a predominant tendency. A special effort was made to secure left-handed children as subjects with the result that 12, 5 girls and 7 boys, were found and tested. It is rather curious that again we find just half of the left-handed group showing the left eye dominant, 3 left-handed girls and 3 left-handed boys.

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[9]The assistance of Mrs. J. H. McCully of the Palo Alto High School, Palo Alto, California, is gratefully acknowledged.

[10]Professor Isa D. Reed of the State Teachers College, Chico, California, was responsible for the careful testing of these children. The work was done at the Stanford Psychological Laboratory in the summer of 1926.

Of these 3 boys, 2 were among those who gave inconsistent or mixed responses, one had 9 *L* and 1 *R*, while the other gave 7 *L* and 3 *R*. The remaining 6 left-handed children showed clear-cut results in favor of the right eye. Twenty right-handed children proved to be right-eyed, while 6 were left-eyed. In 26 out of 38 children tested, that is, 68%, eyedness was found to agree with handedness. As a total group the 38 grade school pupils subdivided into 26 (68%) with *R*-eye and 12 (32%) with *L*-eye dominance. The results on the children agree closely with those reported above on younger and older adults. We may turn now to consider some special groups of adults.

Chinese Group.[11] All 20 Chinese students (men) gave responses that were clearly indicative of the dominance of one eye or the other. Right-eyedness was present in 13 (65%) and left-eye dominance in 7 (35%). Right-handedness was reported for 18 cases with 13 *R*-eyed and 5 *L*-eyed. The two left-handed individuals showed left-eye dominance. The Chinese not only represent a different racial group but they have had a very different reading and writing experience from that of the other subjects tested. Notwithstanding these differences, the Chinese results closely agreed with those for the other sub-groups examined.

Microscope Users.[12] Do users of the microscope thereby develop a persistent ocular habit that amounts to ocular dominance? Ordinarily, medical students are instructed to use the left eye for viewing the magnified preparation, so that the head will not have to be moved when the attention is shifted to the drawing that is on the right-hand side of the miscroscope. Senior medical students should constitute an important group as subjects for eye dominance tests since many of them will have developed a habit of using the left eye for the very reason that they are right-handed.

The total results for 37 medical students (men) show 28 instances of right-eye dominance to 9 left-eye, a ratio of 76 to 24%. The average ratio for the 4 other groups thus far described has been 64 to 34. The number of cases of left-eye dominance among the medical students thus seems smaller than would be expected even

[11]For the most of these data I am indebted to Mr. Siegen H. K. Chou, graduate student in psychology, Stanford University. The data were collected at Stanford.

[12]For assistance in securing these data on senior medical students at Stanford, I am indebted to Mr. Kenneth W. Thompson.

in an ordinary group. Fifteen men reported using only the left eye at the microscope. One of them was left-handed, the others were all definitely right-handed. Of the 15, only 7 showed left-eye dominance. The other 8 gave clear-cut results in favor of the right eye. The men who were accustomed to using the right eye at the microscope numbered 17 (one was left-handed), and of these the *V*-Scope showed 15 right-eyed and 2 left-eyed. Five reported using both eyes about equally at the microscope, and all of these gave results in favor of right-eye dominance. We cannot be sure that the microscope habit has had no effect on the dominance results. But they appear to show about what we ordinarily find in left-handed and right-handed groups. Since handedness is clearly involved in the microscope habit, the results cannot be conclusive except to indicate that the microscope eye is not necessarily the dominant one as revealed by the *V*-Scope.

The results for the medical group as far as the relation of eyedness to handedness is concerned are similar to the results for other groups. The 35 right-handed men show 27 cases of right-eye and 8 of left-eye dominance. The 2 left-handed show one case of each. There are 2 of the right-handed who, according to their own statements, may possibly have been left-handed as children; and here again there is one case left-eye, the other, right-eye.

As before stated, each medical student was carefully questioned about his vision. Among other things he was directly asked if he had a known tendency to use one eye in preference to the other. There were 22 men whose statement of preference agreed with the test findings. In 5 other cases there was direct disagreement, that is, the men said one eye but demonstrated the other. Seven claimed that the eyes were equal. Four of these demonstrated right, and, the others, left dominance.

In connection with these results we may mention that several jewelers have been tested. Here again the special eye habits developed in connection with the requirements of a particular trade cannot be shown to have modified the dominance. Cases have been found of men who are distinctly left-handed in the use of all their tools and who have never had any injury to the left eye, but who, nevertheless, use the jeweler's loupe with the right eye and give consistent right-eye results through the *V*-Scope. It is a fair question whether, when working with such small objects as occupy the at-

tention of a jeweler, the combination of right eye with left hand is not the more comfortable coordination. Combined in this way the hand is not so likely to obstruct the eye's view.

Retested Groups.[13] An important question concerning ocular dominance results in whether individuals retested after a period of weeks duplicated their first performance. A group of 26 university women were tested and then retested after an interval of two weeks or longer. There were 21 who gave just the same results for the second test as for the first, that is, they persisted in the use of the same eye throughout both times. Four subjects may be said to have reversed, that is, changed from one eye to the other. One woman did this absolutely, for in the first test she gave 10 *L* and in the second 10 *R*; another gave first 9 *L* 1 *R* and then 9 *R* 1*L*; a third gave 7 *R* 3 *L* and 10 *L*, while a fourth showed 5 *L* 4 *R* and 10 *R*. There was one case that was wholly doubtful in both tests, 5 *L* 5 *R* and 5 *L* 5 *R*. The retesting of 25 university men gave 20 instances of complete agreement between the first and second series. There was one man who absolutely reversed, and four cases were mixed. Three of the latter each gave 10 of a kind in the second test, but in the first test indications of dominance were not clear. The retest data for the 51 students must be regarded as preliminary only. There is no way at present to account satisfactorily for the failure of 20% of the group to give consistent results. Retest results on children reported elsewhere [Miles (5)] gave almost 95% of agreement. There are some people who have no well-defined eye-dominance habit. Cases of this sort have turned up in some of the other groups as well. It is obvious that such individuals cannot be detected on a single trial as in Parson's test but require a series of tests. In the two retested groups there were 41 individuals who gave decisive and consistent results. The sub-division resulted in 29 *R* and 12 *L*, which is a ratio of 71 to 29%.

Summary

1) The significance of optical dominance is not yet fully evident, although it appears generally demonstrable as a habit.

[13]To Miss Ida B. Squires and Mr. A. Leon Winsor, both working as graduate students in psychology, I am indebted for the data here reviewed. Miss Squires examined the women and Mr. Winsor the men. Both series of observations were made in the summer of 1926 at the Psychological Laboratory, Stanford University.

2) As a rule special tests must be used to detect dominance, and most of these tests are quite subjective in nature.

3) The right hand and right eye are favored in the ordinary alignment test. This defect can be avoided by the use of a special procedure.

4) The *A-B-C* Vision Test described here avoids disadvantages present in the Parson method. It may be repeated many times on the same patient with little interference from handedness and avoids the automatic informing of the patient in regard to findings.

5) As a general rule successive series of tests made on an individual all show the same eye as dominant. In adults right-eye and left-eye dominance occur in the proportion of about 64 and 34%, respectively. About 2% show no well-marked dominance habit.

6) Groups of right-handed individuals show 1/3 or slightly less of their number to be cases of left-eye dominance. Left-handed people are more evenly divided between right- and left-eye dominance.

7) Adults and children show closely agreeing results. Chinese men give results practically identical with those of American men. No significant difference appears for the women as compared with the men.

8) Special habits from training as with the microscope do not determine the eye dominance.

9) Re-examinations after a period of two weeks or longer show identical results in at least 80% of the cases.

10) The right-handed tend rather more to right-eye dominance than do the left-handed to the dominance of the left eye.

11) In about 50% of the cases superior adults know which is their leading or dominant eye. This information has resulted from many experiences in everyday life.

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LA DOMINANCE OCULAIRE CHEZ LES ADULTES HUMAINS

(Résumé)

On peut montrer que la dominance oculaire est une tendance ou habitude chez la plupart des personnes normales. Les spécialistes qui examinent et soignent les yeux devraient être renseignés sur l'habitude oculaire de l'individu soigné. Les tests d'acuité et de brillance monoculaire sont trop subjectifs pour être constants. On devrait adopter quelque méthode qui permet à l'examinateur d'*observer* l'habitude de l'individu. Le meilleur moyen de le faire semble de regarder à travers un cône tronqué (V-Scope) dont l'extrémité large se place devant la figure. Les deux yeux ne peuvent voir en meme temps le même objet de test à une distance de dix pieds. L'examinateur qui a la carte de test voit directement quel oeil le sujet emploie inconsciemment. Chez les adultes on trouve la dominance de l'oeil droit et de l'oeil gauche chez à peu près 64 à 34 pour cent respectivement, tandis que deux pour cent ne montrent aucune habitude de dominance. La dominance de l'oeil et l'usage des mains n'ont pas de rapport nécessairement. On peut s'attendre à ce que le tiers des droitiers montrent la dominance de l'oell gauche et que le moitié des gauchers montrent la dominance de l'oeil droit. Les habitudes spéciales comme celle du microscope ne déterminent pas la dominance oculaire générale. Environ 50 sur 100 adultes

sont au courant de leur habitude de dominance oculaire. Si on observe et note la dominance dans sa rapport avec d'autres observations sur la vision et la coördination, l'importance de cette habitude deviendra de plus en plus claire.

MILES

ÜBER DAS VORHERRSCHEN DES EINEN ODER DES ANDERN AUGES BEIM ERWACHSENEN MENSCHEN

(Referat)

Man kann Vorherrschen (oder Bevorzugung) des einen oder andern Auges bei den meisten normalen Menschen als Neigung oder Gewohnheit demonstriren oder beschreiben. Spezialisten, die Augen untersuchen und behandeln, sollten die Angewohnheiten ihrer Patienten in dieser Hinsicht genau kennen. Tests der Sehschärfe und des Sehvermögens des einzelnen Auges sind zu subjektiv um zuverlässig zu sein. Man sollte eine Methode wählen die es ermöglicht die Gewohnheit des Patienten zu beobachten. Durch einen abgeschnittenen Kegel (*V*-scope) schauen, mit dem breiten Ende gegen das Gesicht gedrückt, dient hierbei wohl am besten. Beide Augen können nicht zugleich denselben Gegenstand in einer Entfernung von 10 Fuss sehen. Der Prüfende, der die Testkarte hält, sieht sofort welches Auge die Vp. unbewusst benutzt. Bei Erwachsenen findet sich ein Vorherrschen des rechten oder des linken Auges im Verhältnis von 64 zu 34, und nur bei 2% findet man keinen Unterschied. Ein Vorherrschen des rechten oder linken Auges wird nicht immer von Rechtshändigkeit oder Linkshändigkeit begleitet. Man kann erwarten, dass ungefähr ein Drittel der Rechtshändigen Vorherrschen des rechten Auges aufweisen und etwa die Hälfte der Linkshändigen, Vorherrschen des rechten Auges. Besondere Angewohnheiten, wie z. B. mit dem Mikroskop, bestimmen nicht das allgemeine Vorherrschen des Auges. Von allen 100 Erwachsenen sind etwa 50 sich der Gewohnheit des Vorherrschens des einen Auges bewusst. Wenn wir dieses Vorherrschen des einen Auges beobachten und verzeichenen in Verbindung mit andern Beobachtungen über das Sehen und Koordination, dann wird die Bedeutsamkeit dieser Angewohnheit noch klarer.

MILES

SHORT ARTICLES AND NOTES

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INDIVIDUATION VERSUS INTEGRATION IN THE DEVELOPMENT OF BEHAVIOR[1]

G. E. Coghill

Integration connotes synthesis; individuation connotes analysis. It has been generally assumed that larger patterns of behavior are formed in the development of the individual by the integration or synthesis of smaller or elementary patterns. Conversely, the thesis of this paper is that smaller or partial patterns arise by a process of individuation or analysis within a larger pattern, and that the primary pattern is a total reaction which normally expands from the first as an integrated process. This thesis has been demonstrated in detail for the development of behavior in Amblystoma by myself (4), and there is evidence from the work of other investigators that it holds also for the development of behavior in lower mammals and in man (3).

Somatic behavior begins in Amblystoma as a contraction of the most anterior part of the muscles of the trunk. As the individual grows older this contraction extends farther and farther tailward till the entire trunk and tail is involved. Particular reactions, also, during this period, begin in the head region and progress tailward. The total reaction of the latter part of this period is acquired, therefore, by expansion of the original pattern; and the only partial pattern in actual performance is an incomplete execution of the total pattern in the order of its genetic development.

This order in behavior is explained by the order of growth in the nervous system. The primary motor mechanism consists of a longitudinal series of neurons on either side (2). These neurons conduct from the head tailward and have branches which form the motor nerves. Their development or differentiation progresses from the head tailward. This explains the cephalocaudal direction of expansion of the total behavior pattern. The

[1]Read before the Ninth International Congress of Psychology.

relation of the motor neurons to the sensory neurons is such that impulses from the sensory field reach only the more anterior part of the motor tract. As a result of this, all responses to exteroceptive or proprioceptive stimulation begin in the anterior part of the muscular system and progress tailward, and consequently all partial patterns of behavior of this period are simply abbreviations of the total reaction.

As the appendages develop the total pattern expands into them with the result that at first they move only as the trunk moves. A little later they move as part of a postural pattern and take definite positions or attitudes according to the posture of the trunk. Still later the appendages acquire patterns of their own which, when performed in response to a local stimulus, are known as reflexes. Discrete limb movement is therefore acquired by a process of individuation within an expanding total pattern. The process is one of analysis, not of synthesis.

That integration is a primary condition through all the motile parts of the embryo is explained by the fact that the motor neurons that control the functional muscles grow by means of new branches into non-functional muscle primordia. When muscles arise from the primordia, they find themselves under the dominance of the mechanism of the total behavior pattern. The total pattern thus maintains its supremacy over partial patterns that arise from time to time within it.

That the reflex is in its genesis dominated by the total behavior pattern is further demonstrated by the fact that before reflex movement of the arm in response to exteroceptive stimulation can be elicited when the animal is in its usual resting position, it can be excited when the animal as a whole has assumed an unusual posture. At the stage of development in question Amblystoma rests with the ventral side down and with the two arms in like position relative to the body; but if the animal is turned upon its back and with the trunk and tail slightly twisted upon the longitudinal axis, the arm on the side that is rotated downward is elevated and abducted as if reaching for the bottom of the receptacle, while the other arm is depressed and adducted to a position close to the body. If, now, the elevated and abducted arm is lightly touched it quickly elevates and abducts without any other perceptible action by the animal. In this case, posture is seen, not as a synthesis of reflexes, but as a total reaction, and as such it sensitizes a nascent reflex mechanism to exteroceptive stimulation. Here the reflex is a discrete thing overtly, but inherently it is indissolubly in alliance with the total behavior pattern.

In the reaction which has just been described the posture is overt. Later, when the reflex occurs while the animal is in the resting position, there is reason to believe that there is a nervous equivalent of posture which does not express itself overtly in muscular activity. This belief is founded in two facts: first, that for a considerable period before the arm reflex can be

elicited, local stimulation of the arm inhibits the total reaction, whereas formerly it excited the total reaction; secondly, that in the genesis of the arm reflex there is a progressive reduction of the trunk component of the total reaction until the arm movement becomes overtly a discrete local reaction. Inhibition, therefore, begins as a total pattern and as such in the beginning dominates the whole animal, whereas in its later phases of development it may give place more or less to local excitation and permit the appearance of local reactions. According to these facts inhibition holds the same relation to the genesis of the reflex as does overt posture. Inhibition may, then, be regarded as concealed posture, to which reflexes in general behavior hold the same relation that the earliest discrete reflexes hold to overt posture. In other words, every definitive reflex is the overt excitatory component of a total pattern that includes the inhibition of all quiescent parts.

The general law of individuation of reflexes out of a total pattern, as I have observed it in Amblystoma, is now recognized in the development of the behavior of the albino rat by Angulo y Gonzalez (1). In a contribution now in press this investigator describes the earliest movements of the fore limbs and jaws of the rat as components of a total pattern with movement of the neck and trunk. Only in later phases of development does he observe discrete action of the limb, or opening and closing of the mouth without other overt responses. Also, a study of all cases on record of direct observation of movements of young human fetuses indicates that the law holds for man (3). It has not been recognized, however, by the investigators in this field.

The movements of at least 50 human fetuses of 6 months and younger have been put on record by various observers, including Minkowski, Bolaffio and Artom, Erbkam, Krabbe, Strassman, Zuntz and Yanase.[2] It is from these authors that I have drawn my conclusions concerning the development of reflexes in man.

Arm movement and leg movement have been observed in human fetuses of about six weeks by Straussmann and Yanase. The spontaneous movements of a fetus of about two months are described by Minkowski as vermicular, involving the body, head, arms, and legs. The earliest movements of the fingers that have been observed occurred in a fetus of an age estimated at a little less than three months. But before the behavior pattern has expanded into the fingers certain reflexes have made their appearance.

The earliest oral reflex in response to touch on the lip involves not only the closing of the mouth but also movements of the legs. In other words, the reflex extends throughout practically the entire length of the medulla oblongata and spinal cord. This occurred in a fetus of about two months (3.5 cm), whereas closing of the mouth without movement of other parts in

[2]Specific references to these authors are given in (3).

response to a touch on the lip occurred first in a fetus of about 18 weeks (21.5 cm.). Likewise the earliest contraction of the muscles of the eyelid in response to touch on the lid occurred with action of both legs. This was at the age of about 10 weeks, whereas discrete action of the muscles of the eyelid in response to touch on the eye first occurred at the age of about 18 weeks. The earliest recorded response to touch on the anal mucous membrane occurred at about 14 weeks (13 cm.) with strong movements of the legs, whereas contraction of the anal muscles only in response to such a stimulus is not recorded for fetuses under six months. The respiratory movements of a fetus of about four months (16 cm.) are described by Erbkam as opening of the mouth, movements of the head from side to side, and movements of the arms and legs; and Minkowski describes the respiratory movements of a fetus of about 18 weeks (21.5 cm.) as convulsive. It seems certain, therefore, that the oral reflex, the eyelid reflex, and the anal reflex arise by individuation within a much larger pattern of reaction, and that the definitive respiratory reflex arises by individuation within practically a total reaction pattern of the early fetus.

The genesis of the plantar reflex is for many reasons of special interest (5). The earliest response to stimulation on the sole of the foot is not a simple movement; it involves extension of the leg and dorsal flexion of the foot. This occurred at about 10 or 11 weeks (7.5 cm.). At approximately 12 weeks (8.5 cm.) this reflex involves flexion and rotation of the foot with flexion of the toes. It should be emphasized that the reflex action of the foot in this case was not a simple movement; it was a fusion of flexion and rotation. The earliest reflexes of the toes involve all the toes integrated with movement of the foot. Spreading of the toes in response to stimulation on the sole is fused with flexion of all the toes. It is only at about 16 weeks (16 cm.) that the great toe begins to acquire independence of the other toes in the plantar reflex; and the early phase of its independence is expressed, not by acting even weakly as an independent organ, but by acting more strongly than the other toes in a common reaction. Complete independence of action of the great toe is acquired at about 17 weeks (18 cm.), when it performs dorsal flexion while the other toes undergo ventral flexion. This is the earliest performance of the Babinski sign on record, and it is clearly the end product of a progressive individuation within the total action-system of the leg, foot, and toes.

Conditioned reflexes, which are generally accepted as the units out of which behavior develops synthetically in postnatal life, are generally regarded as differing in their genesis from the unconditioned reflexes of fetal life. It is probable, however, that this hypothesis is not well founded. Pavlov's dogs which condition their reflexes in an orderly way do so by progressive reduction of the field of adequate stimulation for the particular reflex and by progressive restriction of the motor field of action. These

characteristics the conditioned reflex has in common with the unconditioned reflex. But some dogs when subjected to the Pavlov experiments go to sleep, while others become greatly excited. In the one case, sleep is a total reaction in the phase of inhibition; in the other case, the excited activity approximates a total reaction in the phase of excitation. These dogs which begin their response to the conditions of the experiment with virtually total reaction may eventually condition their reflexes appropriately. This appears to be accomplished by establishing a balance between inhibition and excitation which permits the emergence of the conditioned reflex. This balancing of inhibition and excitation is exactly what occurs in the genesis of unconditioned reflexes as I have seen it in Amblystoma.

I conclude, therefore, that the conditioned reflex, like the unconditioned, is acquired by analysis of a total pattern which under normal conditions is from the beginning perfectly integrated.

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THE ESSENTIAL NATURE AND DEFINITION OF REASONING

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It is our purpose in this paper to indicate several criticisms of the present-day psychology of reasoning (especially as presented in the textbooks) and, at the same time, to offer several suggestions which are intended to be constructive. The central point of the following discussion is the problem of the essential nature and definition of reasoning.

During the past five years the writer has systematically and critically studied the treatments of reasoning in the various special books on the subject. He has likewise made a comparative study of the treatments of reasoning as given in practically all the textbooks of psychology as well as the various theoretical and experimental articles reported in the journals. As a result he has that same "indefinable sense of uneasiness

and discontent" into which Rignano (15) says that he was thrown by "the perusal of some of the best treatises of logic." Some of the chief causes for this "uneasiness and discontent" will be discussed in the following pages.

In the first place, there is still too much confusion between the logic and the psychology of reasoning. In 1924, Pillsbury (14), in an excellent statement of modern theories of the psychology of reasoning, pointed out the present-day naturalistic trend in the discussion of reasoning. In spite of this naturalistic trend, of which Pillsbury speaks so optimistically, we still read of syllogisms, degrees of "truth," and many other items which ought to belong to a logic textbook. In 1896, Stratton (17) wrote a brilliant paper which sounded a clear declaration of independence for a psychology of reasoning as against a logic of reasoning. Apparently some psychologists do not agree with Stratton's arguments for a complete separation of the logical and psychological treatments of reasoning. After all, why should Rignano have been discontented with the psychology of reasoning in the "best treatises of logic"? One should not expect to find a psychology of reasoning in a textbook of logic, although the writer notes that most books on logic confuse the materials of logic and of psychology.

But, coming directly to the expositions of the psychology of reasoning, our second cause for "uneasiness and discontent" lies in the present confusion of thinking and reasoning. At present the two terms either are used interchangeably or else are only vaguely distinguished. Some psychologists have a chapter on thinking and never mention the word reasoning.[1] Other writers label their chapter "Reasoning" and do not use the term "Thinking."[2] A few writers are making the proper distinctions. For example, Gates (5), in a recent textbook, groups a series of related processes under a chapter entitled "Reasoning, Imagination, and Other Types of Thinking." One notes that up to and including the year 1924, there was a separate heading in the *Psychological Index* for reasoning. In 1925 this heading was entirely dropped and the term reasoning is no longer used in the classification of published work which formerly would have been placed under this title. Only the heading "Thought" is now used. Inasmuch as the classification used in the *Index* has the sanction of the American Psychological Association, this change appears to have considerable significance.

The writer believes that the two terms should be kept distinct and that their proper relationship should be made as clear as possible in any exposition which an undergraduate college student is expected to study. Thinking is the broader term, and reasoning is a special form of thinking. In a word, all reasoning is thinking, but not all thinking is reason-

[1]For example, see (1) and (6).
[2]For example, see (12) and (3).

ing. Unfortunately, the proper use of the term thinking is not well agreed upon.

If an association process takes place under the limiting and guiding influence of what is variously termed the problem, purpose, *Aufgabe* and the like, the process will very likely be classed as thinking. In other words any process of so-called controlled association is thinking.[3] This statement seems to be clear enough until a more searching inspection is made. Upon close scrutiny, one finds that a problem-consciousness, purpose, or Aufgabe, considered as a conscious or neurological structure, is a very ill-defined proposition. On the conscious level one may safely say that the attitude-consciousness of the task may vary from a fairly limited and specific mental structure to a most vague and general state of mind. In other words, the "control" may vary from a specific mental state, such as an image in mind, to a vague and indefinite attitude.[4]

The "control" of the association process may be exerted by a neural organization which operates on an unconscious level. In fact, there is much evidence that unconscious influences are constantly at work exerting a guiding and limiting influence on our association processes.

In the light of the above considerations, one may seriously question the wisdom in speaking of uncontrolled association. There would seem to be no such thing as uncontrolled association, and the suggestion offers itself that any distinctions must be but arbitrary limitations based upon degrees of specificity and vividness of the conscious state (purpose, *Aufgabe*). Revery, daydream, and dream would not be classed, ordinarily, as thinking because they do not seem to be carried out under a prevailing consciousness of task or purpose. On the other hand, one might make a strong case in favor of their being called thinking processes in which the control is simply less specific and conscious.

If reasoning is considered as a special and unique form of the thinking processes, one must point out the nature of the process which furnishes the distinctive feature or features. This constitutes our third source of dissatisfaction with the present psychology of reasoning. Different writers do not agree in their definitions of reasoning.

In general, one finds two types or forms of definition of reasoning. In one type of definition the process is emphasized. That is to say, some peculiarity or uniqueness in the association process is indicated as the distinguishing feature of reasoning. We may call this the "process"

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[3]J. E. Miller (9), in his book on *The Psychology of Thinking* (1925), is one of the few writers who gives adequate emphasis to the distinction between thinking and reasoning.

[4]In his book, *Attention* (1908), Pillsbury (10) develops this notion of relative specificity and immediacy in connection with his discussion of the subjective conditions of attention. The image in mind is the most immediate and specific condition, the attitude is a more general organization, and behind these lie the effects of training and heredity.

form of definition. On the other hand, some psychologists hold that there is no distinguishing feature about the association-movement. The distinguishing feature for them lies in the attitude which is taken toward the outcome of the process. This form of definition may be called the "end-result" type of definition, in the absence of a better designation. A third type of definition, which has much in common with the last-named form, might be proposed, but, as far as the writer knows, has not been used. In this form of definition one might say that the characteristic feature of reasoning lies in a peculiar state of mind which initiates the thought process. As such this form of definition becomes something of an antithesis of the end-result form.

In at least one instance Pillsbury[5] (13) gives a definition of reasoning which may be classed as an end-result definition. Thus, he says, "A very simple definition is that reasoning is purposive thinking. But to constitute reasoning thinking must not only be purposive, i.e., must have a definite end, but must also be true and able to justify itself. Reasoning is a process of solving problems. The solution of the problem must be true and must also be proved true" (p. 242).

Pillsbury apparently believes that any search for uniqueness in the association process is futile and so emphasizes the attitude which is taken toward the result of the process. He goes on to say, "Reasoning may be distinguished from memory and imagination, not so much by the character of the mental states or by the way they are obtained, as by the attitude taken toward them when they arise." The statement is then added that " . . . . the results of reasoning are new and are accepted as true; the results of memory are true, but not new; and the results of imagination are new, but not true" (p. 242).

It is possible, of course, that Pillsbury is right in denying any distinguishing features to the reasoning process. On the other hand, the matter is by no means agreed upon, and even Pillsbury himself launches into an analysis of the thought-process with the result that there emerge three distinctive features of the thought-cycle, namely, judgment, inference, and proof. Most persons would probably insist that they had reasoned even if their conclusion was very incorrect and unbelieved. Indeed, one may well raise the question as to whether or not the psychologist has any right, as a psychologist, to speak of any mental structure or function as true or untrue. When we, as psychologists, begin to speak of any mental structure as true or false, are we not infringing upon the domain of logic?

Little need be said about the type of definition which emphasizes the nature of the initiating and limiting circumstance of mind. A reasoning process is initiated and guided by a problem-consciousenss, a lack of

[5]In other works this authority gives more emphasis to the process part of reasoning.

mental harmony, or mental conflict. On the other hand, a mental conflict or disharmony may result in mere revery, dissociation, or even sleep.

As samples of process-definitions we may refer to the expositions which have been given by James (7) and Binet (2), two independent workers who arrived at very similar definitions of reasoning. James says, "Let us make this ability to deal with novel data the technical differentia of reasoning. This will sufficiently mark it out from common associative thinking" (p. 330). James then goes on to say that reasoning involves analysis and abstraction, making it a definite ideational and conscious process. He then defines reasoning as....."the substitution of parts and their implications for wholes," and points out that reasoning involves association by similarity in contradistinction to association by contiguity. While some of James's language sounds a bit metaphorical, and although he perhaps over-emphasizes the ideational aspects, his grasp of the process-movement is masterly, and little advance has been made over his analysis.

Binet's analysis is consistently psychological. Like James, he stresses association by resemblance. In the total reasoning process there is first association by similarity and then association by contiguity. Reasoning is thus defined: "Reasoning is the establishment of an association between two states of consciousness, by means of an intermediate state of consciousness which resembles the first state, which is associated with the second, and which, by fusing itself with the first, associates it with the second" (p. 15).

As a result of such disagreements as those which we have indicated above, a fourth unfortunate condition obtains in the exposition of the psychology of reasoning. Some writers use the term reasoning for a complete act of thought, whereas other writers mean any one of a large number of processes which may be involved in the total thought-cycle. Dewey (4), for instance, finds five stages in his complete act of thought. Pillsbury (11) finds three stages in his analysis, namely, judgment, inference, and proof. As a matter of fact one may note reasoning, defined in terms of process, in any one of Dewey's or Pillsbury's steps. It would seem desirable to indicate the distinction between a total thought-movement or cycle and a reasoning process. The flashes of "insight" or the consciousness of suddenly perceived relations come at any and all stages of the thought-cycle, and each "flash" is a reasoning process in itself.

The following definition of reasoning is suggested: Reasoning is a process which may occur at any point in a thought-movement and consists in the appreciation (on a conscious or unconscious level) of likenesses and differences between old experiences and a new situation. The definition differs from that given by James and Binet, for example, in that it emphasizes the noting of differences as well as likenesses. This suggestion is to be accredited to a very stimulating work reported in 1913 by Shepard and Fogelsonger (16). On the basis of careful analysis of very keen introspective reports, which they secured from their subjects in a study of association processes, these writers conclude, "....and this analysis of differences as

well as likenesses seems to be the essence of the reasoning process." In their analysis of the shiftings and regroupings of elements of experience they report that "it is desired to suggest here that the interassociations among the elements of a group have their inhibitory side of the kind shown in our experiments, which causes the factors of a sub-group to hold together and at the same time seem incomplete, which prevents mixture and confusion of groups, and which consequently, in the alternation from one group to another, forces a distinction of the differences from the likenesses up to the limit of this type of ability of the individual" (p. 310).

In the thought-movement, in which reasoning may occur at any one or more points, there is, then, an *Aufgabe* or problem in mind which initiates and exerts some guiding, selective influence on the course of the association process. At some point in this thinking process there is a recombining of elements of past experience into a new mental pattern. At this point there is reasoning. If a human is concerned and is working a puzzle or problem, he suddenly exclaims, "There! I see it." At this point we speak of "insight" or "seeing the point," or "grasping the situation." At this point one applies one's past experience in a new way to meet a novel situation.

Granting that our definition or concept of reasoning has some degree of truth in it, another perplexing question arises. This question constitutes a fifth source of discontent in the mind of the writer. Unfortunately, there seems to be no way of alleviating this discontent. The question which arises is: How shall we know, in any given case, whether the man, infant, or animal is reasoning? Even the behaviorist has failed to give us a satisfactory objective criterion of reasoning! The need for some definite, infallible objective sign of reasoning is one of the most pressing needs in this field of psychology.

Some reasoning involves little or no motor activity when the difficulty to be overcome is not an objective situation. On the other hand, some reasoning involves an objective situation and motor adjustments. Man seems to have a monopoly on the first kind of reasoning, as there is little evidence that any lower animal ever sits down to think over some abstract problem. Just how shall we know when our thinker has a flash of insight or reasoning? At present there is no objective criterion.

Whenever the situation which arouses the problem-consciousness is objective and demands some series of movements for the solution of the problem, the situation appears to be more hopeful, although at present no adequate objective criterion can be offered. When Lloyd Morgan's dog, carrying a a long stick in its mouth, succeeded in getting through a small hole in a picket-fence, the question must have arisen at once: Did the dog reason? The solution of the problem might have been sheer accident, as a result of a trial-and-error process. Again, the animal may have used an old habit, having learned on a previous occasion how to get through a hole in a fence when carrying a long stick in its mouth. Finally, the dog may have reasoned. But how shall we know unless we know the complete life history of the animal?

A suggestion which is in no sense new may be derived from Köhler's observations of apes. The apes manifested certain characteristic outward behavior when they had "insight." We may refer to but one instance where the ape, Sultan, was trying to reach a banana which was suspended overhead beyond his reach. In order to reach the banana a smaller box had to be placed on top of a larger one. After placing the first box in position the animal apparently failed to appreciate the usefulness of the second box which lay to one side. A fairly typical trial-and-error process then took place. Then, out of this trial-and-error process which involved running around and kicking, along with a show of temper, there came a sudden insight. Köhler (8) writes, "But all of a sudden this behavior changes completely; he stops making noise, pulls his box from quite a distance right up to the other one, and stands upright on it. He mounts the somewhat shaky construction....." (p. 136). Note, in this case, how the ape *ceases his random movements, hesitates, and then at once executes the proper movements.* A study of motion-pictures of rats learning mazes and of college students working block-construction puzzles has convinced the writer that all show similar motor reactions at the time of insight. Inhibition of random movement, hesitation, and a direct execution of the proper movements seem to be the essential objective signs of reasoning.

Summary

We have tried to indicate some of the conditions in the present-day psychology of reasoning which are unfortunate, and we have made certain positive suggestions for the changing of these conditions.

First, it has been pointed out that a clean-cut distinction should be made between the logic of reasoning and the psychology of reasoning.

Secondly, it has been noted that there is much confusion in the use of the terms thinking and reasoning. It has been suggested that the terms be kept distinct and that reasoning be considered as a process which occurs in thinking.

Thirdly, it has been pointed out that different writers give very different definitions of reasoning. Some emphasize the process whereas others stress the initiating consciousness or else the end-result and attitude. It has been suggested that a process definition is the proper kind to emphasize.

Fourthly, it has been stated that writers disagree on the amount of any process which is necessary in order to classify the process as reasoning. It has been suggested that a distinction be made between a thought-cycle or act and a reasoning process. A definition of reasoning has been offered as follows: Reasoning is a process which may occur at any point in a thought-movement and consists in the appreciation (on a conscious or unconscious level) of likenesses and difference between old experiences and a new situation. We have referred to the work by Shepard and Fogelsonger as throwing light upon the more ultimate nature of the reasoning process as such.

Fifthly, we have noted the need for an objective criterion of reasoning. It has been suggested that, wherever action is involved, a cessation of ran-

dom movement and hesitation and the direct execution of the proper movement may be considered as objective criteria of reasoning. We appreciate the fact that these criteria are not infallible and may, at times, be misleading.

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THE INTERPRETATION OF AUDITORY STIMULI OF VERY LOW INTENSITY[1]

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The purpose of the experiment to be described in this article was to investigate the variability and educability of individuals in their interpretation of very slight sensory stimuli, in this case auditory, which are usually not present as conscious factors in our formation of judgments.

[1]I wish to express thanks to Dr. S. I. Franz for his suggestion of the problem and for his help and advice throughout the investigation.

The tests were made to determine whether or not generally untrained subjects could select from a "sound mass" elements which are usually unattended to, and at the same time to discover to what extent such subjects could read meaning into these elements. The experiments to be reported have dealt with sound elements which are complex and common, but they point to the need of further experimentation on problems of the discrimination threshold where the sounds are simpler and less massed and also where the sounds are more disguised. In general, the problem is similar to that raised by the question, "How does the conductor of an orchestra select from the sound mass those tones produced by one performer?" or, to another question, "To what extent does the blind subject select those sounds which enable him to make the judgment that he is passing a picket fence?"

For the first part of the experiment, 15 volunteer subjects, university students, were used.

The subject sat at a table in a small cubicle, about 6′ x 11′, facing a closed window, with his back to the experimenter, who stood 1′ 10″ behind. An introductory explanation was given to the subject, about as follows:

"I want to find out how correctly you can make judgments as to what I am doing. For the purpose of simplifying the experiment, I shall make one of six movements, and you will write down the movement you think I have made. I shall stand behind you so that you cannot see what I am doing, and I shall move my right or left arm in one of three directions: forward, backwards, or sidewise away from my body. I shall tap a pencil just before and just after making a movement, then you are to make a judgment. In order that we may determine how well you are able to do this, I want you to add to your judgment the letter *C* if you are quite certain that you are correct, the letter *F* if you feel fairly certain, and the letter *G* if your judgment is only a guess. In every case you are to make a judgment, even though they may all be guesses."

In this part of the experiment each subject made judgments of 30 movements, that is, five repetitions of six possible kinds of movement. The order in which these were performed was determined each day by writing each of the six possibilities on five different cards and then shuffling the 30 cards. The light, shining as it did in the face of the subject, precluded the possibility of shadows which might serve as visual cues in the interpretation of movements performed. Since 15 subjects were used, there was a total of 450 judgments. Of these 108 were correct, where 75 would have been expected simply according to the laws of chance.

Table 1 shows in more detail the nature of the results, where it is of interest to note that, by the law of chance, five would be the number of correct judgments to be expected from each subject.

After each experiment the recording of judgments was followed by a brief introspection on the part of the subject as to the reasons for certainty or fair certainty, if such existed. Table 2 gives the number of correct judg-

ments made by each subject, together with a brief statement of the introspective account.

An examination of Table 2 definitely shows that many of the subjects ascribed their judgments to auditory cues, although care had been taken

TABLE 1

NUMBER OF SUBJECTS MAKING DIFFERENT NUMBERS OF CORRECT JUDGMENTS

No. of subjects	No. correct judgments per subject	Total number correct judgments
1	3	3
1	4	4
1	5	5
1	6	6
5	7	35
2	8	16
3	9	27
1	12	12
15		108

Total number of tests: 450

Average number of correct judgments per subject: 7.2

Average number of correct judgments to be expected by the theory of probability: 5.0

TABLE 2

Subject	No. of correct judgments	Account given
1	9	"Just an idea; the first idea I had."
2	8	Sounds; quality and time of rustling of dress.
3	4	Sounds; also length of time between taps.
4	9	Sounds.
5	7	Sounds.
6	7	Sounds.
7	8	Sounds; "then sometimes I could see a picture, a little flash."
8	12	Sounds.
9	7	Sounds.
10	7	Practically no cues; once in a while a rustle.
11	3	"Could kind of see it." (Admitted on being questioned sometimes noticing differences in sound.)
12	6	Sounds.
13	9	"Saw a picture; saw a movement in imagination."
14	5	"Could just get a picture of the thing."
15	7	Sounds.

TABLE 3
NUMBERS OF CORRECT JUDGMENTS OF SUBJECT 8 IN RELATION TO PRACTICE SERIES

Trial	Series 1 Prior to Practice	Series 2 Immediately following practice	Series 3 Week following practice
1	13	23	15
2	11	17	17
3	7	18	15
4	10	15	18
Average	10.25	18.25	16.25

by the experimenter not to suggest this in advance. The experimenter wore a silk dress or a shirt waist and sweater, the rustlings of which presumably furnished the hint as to which movement had been made.

The next step in the experiment consisted, then, in the further exploration of what appeared to be interpretations of slight auditory stimuli and an investigation as to the amount of educability possible. For this purpose Subject 8 was chosen, whose number of correct judgments in the first experiment had been 12, exceeding by 7 the number to have been expected according to the laws of chance. She explained her judgments altogether on the basis of sounds.

The above-described experiment was now repeated with Subject 8 on four successive days, a difference order being used for each day.[2]

This series was then followed by experiments on four successive days in which a practice period preceded each regular experiment. In the practice period the subject made oral judgments of movements 1-15 in the series of 30, and was told after each judgment what movement had actually been made. Before movements 16-30 the subject was told in advance which movement was about to be made and had thus an opportunity to observe carefully any variations in sound. The same series of 30 movements was then repeated, movements 1-15 being told in advance and movements 16-30 being judged first and then told to the subject. At the conclusion of the practice period each day the regular experiment was then performed to test the subject for immediate retention, using, of course, a new order of the movements.

This was followed by a third series like the first, with Subject 8, to test the permanent results of such learning as the practice period might have produced. This series was carried out exactly as the first on four successive days.

The results of these three series are given in Table 3.

[2]The subject had been told of her number of correct judgments in the earlier part of the experiment and in the first of this series of four experiments.

The same clothes (cotton shirtwaist, silk shirt, wool sweater, and scarf) were worn by the experimenter throughout these three series that were worn in the original experiment with Subject 8 on March 16, 1928. For the purpose of determining the part played in the formation of judgments by the slight auditory stimuli involved in the rustling of the clothes, a series was next performed on four successive days in which the experimenter wore only a bathing suit. All the other conditions of the experiment were the same as in the earlier series. However, practically all sound was thus eliminated. In this series the numbers of correct judgments were 5, 3, 2, and 10 respectively, with an average of 5. The subject asserted that she had no cues of any kind, and the average is seen to be 5, which is precisely the number correct to be predicted according to the laws of chance.

The attempt was next made to repeat a similar series, performing the experiment in a bathing suit, but with both subject and experimenter at the foci of parabolic reflectors placed 58 inches apart. It was believed that the amplification by the reflectors of such sounds as were produced by movement of the arms might be sufficient to make them significant factors in forming judgments. An effort was made to get the shoulder of the experimenter at the focal point. Because of the size of the reflectors, it was necessary to perform the experiment in a larger room, so conditions in this respect were not the same as before, and had the results been more decisive, a control series would have been repeated under the same conditions. In this series the correct judgments given were 6, 8, 6, and 5 on the four successive days, with an average of 6.25. This average, 6.25, is hardly more than could have been expected simply by chance in so brief a series. The spatial difficulties because of the shape of the reflectors were such that this series in the experiment was not at all satisfactory nor conclusive.

The possibility now suggested itself that the superiority of Subject 8 in the original experiment where 15 subjects were used and her later marked improvement during the learning period might be due to superior sensitivity of her hearing equipment. The hearing of the 15 subjects was therefore tested by means of an audiometer, right and left ears, for frequencies 64, 128, 256, 512, 1024, 2048, 4096, and 8192. There was no indication that among the 15 subjects a high score in correct judgments resulted from special auditory acuity. At every pitch tested, for example, Subject 8, whose number of correct judgments was in the first experiment 12, was inferior in acuity of hearing to Subject 11, whose correct judgments numbered 3.

The next stage in the experiment was performed in the fall term of 1928 (Oct. 15–Nov. 1) in an effort to determine whether the educability and marked improvement of Subject 8 in her judgments had been for any reason an isolated phenomenon or could be expected to occur with any normal subject and even under somewhat altered conditions. For this further in-

vestigation 6 subjects were used, 3 of whom had been among the original 15 subjects.

The procedure with these six was identical for the most part with that used in testing and "educating" Subject 8. That is, with each of these a series of experiments the same as described above was performed on four successive days (Series 1), under the same conditions as before.[3] This series was followed by Series 2, experiments on four successive days in which a practice period preceded each regular experiment. These practice periods were exactly the same as those for Subject 8 described above, and were followed by the regular experiment each day to test each subject for immediate retention, using, of course, a new order of movements each time. There followed then a third series on four successive days (Series 3), like the first, to test the permanent results of such learning as the practice period might have produced.

However (and this was not included in the experiments with Subject 8), each day at the conclusion of this experiment a still further series was performed in the large room (about 13′ x 16′) outside the cubicle.[4] Here the experimenter stood at the same distance behind the subject as before and controlled carefully all visual cues. But sound conditions were very different, many loud noises from the hall entering through the transom and the thin walls, a hot air furnace roaring rather constantly, and reverberations being very marked.

The results are shown in Table 4.

Figure 1 is a graphic presentation of the results tabulated in Table 4.

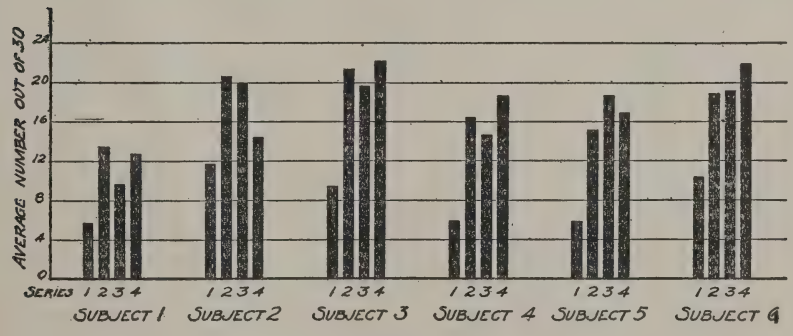

FIGURE 1

[3]Throughout these experiments the experimenter wore a black silk dress and silk scarf, which had been worn also during the experiments with Subjects 1, 2, 3, 4, 5, and 6 of the original 15.

[4]The room in which the experiment with the parabolic reflectors was carried out.

It indicates clearly the marked difference in levels of the averages before and after the "learning" period.

We see, then, not only in the case of the original Subject 8, but also in the case of these six other subjects, the phenomenon of "learning" in the

TABLE 4

NUMBERS OF CORRECT JUDGMENTS OF SIX SUBJECTS IN RELATION TO PRACTICE SERIES.

Series 1: Prior to practice

Trial	Subject 1	2	3*	4*	5	6*	Total
1	8	15	9	9	7	9	57
2	4	13	10	4	5	8	44
3	8	10	9	5	5	15	52
4	3	9	10	6	7	10	45
Total	23	47	38	24	24	42	198
Av.	5.75	11.75	9.5	6	6	10.5	49.5

Series 2: Immediately following practice

Trial	Subject 1	2	3	4	5	6	Total
1	12	22	16	15	13	15	93
2	14	18	22	23	15	18	110
3	16	23	25	13	16	25	118
4	12	20	23	15	17	18	105
Total	54	83	86	66	61	76	426
Av.	13.5	20.75	21.5	16.5	15.25	19	106.5

Series 3: Week following practice

Trial	Subject 1	2	3	4	5	6	Total
1	11	18	17	12	17	19	94
2	10	24	22	17	17	20	110
3	6	20	19	14	16	18	93
4	12	18	21	16	25	20	112
Total							
Av.	9.75	20	19.75	14.75	18.75	19.25	102.25

Series 4: Week following practice, in large room

Trial	Subject 1	2	3	4	5	6	Total
1	12	15	24	10	11	19	91
2	15	11	18	24	14	25	107
3	12	13	22	16	23	18	104
4	12	19	25	25	20	26	127
Total	51	58	89	75	68	88	429
Av.	12.75	14.5	22.25	18.75	17	22	107.25

*These had been subjects 6, 9, and 13, respectively, of the original 15.

interpretation of very slight auditory stimuli. From the average total of 49.5 correct judgments before practice, we have a rise to 106.5, 102.25, and 107.25 as the average numbers of correct judgments in the three experiments after the practice period.

The difference between the results of the last two series, in the cubicle and in the large room, respectively, is probably not significant. It is possible that in the small cubicle the echoes were slightly more confusing than in the final experiment in the large room. Subjects 1, 2, and 5, however, expressed themselves as finding the large room the more difficult one in which to make judgments. Subjects 3, 4, and 6, on the other hand, claimed to find it easier to make judgments in the large room, especially as to whether the movement made was on the right side or on the left. In any case there is some evidence of retention of this habit of response under conditions slightly different from those under which the response was learned.

Summary

Fifteen subjects, knowing in advance that one of 6 possible movements was to be made behind their backs, made a larger number of correct judgments out of 30 (average 7.25) than was to be expected by the laws of chance (5), ascribing their judgments in the majority of cases to auditory cues.

One subject selected for further investigation, after a four-day practice series, made much higher averages in two four-day series (18.25 and 16.25) than she had made before practice (10.25). However, when auditory cues were now eliminated, her average dropped to five, which was to be expected by chance.

Six other subjects trained in the same way gave similar results after a four-day practice series. (Total average prior to practice, 49.5; after practice 106.5, 102.25, and 107.25.)

Conclusions

The conclusions to be drawn from this rather simple experiment are perhaps somewhat obvious. It is apparently quite generally possible to observe and interpret carefully many of the very slight stimuli which are not ordinarily consciously present in the formation of judgments. That they are present as subliminal sensory factors of which individuals are not usually conscious because of the other more readily accessible sources of information is suggested but not established. That conscious attention can be directed to them and marked improvement made somewhat rapidly in their interpretation seems to be proved definitely by the results of these experiments. This fact has a useful application in work which can be done in the education of the blind. They are apparently better able than the normal subject to select from the "sound mass" those elements which

enable them to make accurate judgments, because the necessity has been forced upon them of attending to those factors which are usually disregarded by individuals whose vision is reliable. The blind subject, then, has trained himself and can still further train himself to select, attend to, and interpret the very slight auditory elements which compose the mass, just as the orchestra conductor after years of training can solve the problem of selecting from the "sound mass" those tones produced by one performer. The situation has its visual counterpart in the acuteness of observation in lip-reading among the deaf. Here it is the minute visual details which must be selected and interpreted. The rapid response to training observed in such experimentation as has been described here suggests the possibility of a high degree of educability in the utilization of many of the sensory stimuli which are usually disregarded.

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WEISS, A. P. *A Theoretical Basis of Human Behavior.* (2nd ed., rev.) Columbus, Ohio: R. G. Adams, 1929. Pp. xvii+479.

With the continued loss of interest by the students of human behavior in the study of those hypothetical mental processes which constitute the subject-matter of psychology, there has arisen a very important need for the formulation of the fundamental outlines of anthroponomy, the science of human behavior. Ogden, in a recent interview (*J. Genet. Psychol.*, 1930, **3**) of Boring's *History of Experimental Psychology* and of the *Foundations of Experimental Psychology,* seems to see the present bankruptcy of psychology and appears to believe that those who now called psychologists should either affiliate themselves with philosophy or with biology. This real or assumed dispair of Ogden's is partly due to the failure of the anthroponomists to emphasize sufficiently the scope of their own domain as separate from biology and partly due to the fact that students of human behavior are more interested in problems than in adhering strictly to the prescribed limits of a science. It is this latter reason which has always led the *Psychological Index* to cite all titles bearing upon the problems of human behavior and "consciousness," irrespective of whether or not the authors are psychologists. This policy is continued in the *Psychological Abstracts,* and it is illustrated in a striking way in the *Child Development Abstracts* where one finds titles from medicine and biology which could in almost no degree be called psychological or behavioristic. The concentration of the student of behavior upon the solution of his problems without regard to the scientific border lines which he must cross is inevitably attended at first with a failure on the part of others, and sometimes on the part of himself, adequately to envisage the essential domain of his science.

Weiss, in the present book, has given one of the most thorough analyses of the general assumptions and the fundamental problems of human behavior which we have. There is no attempt to summarize and weight

experimental results, and this seems to me a defect. It would be decidedly helpful to have Weiss's judgment on just what experimental material is peculiar to the field of anthroponomy and to see how he would relate this material to his fundamental concepts.

The book is divided into three parts. The first deals with the fundamental principles of behaviorism. Here the author points out that behaviorists, like all scientific men, depend upon physics and chemistry for their knowledge of the nature of the world. At the present time, electrons and protons seem to be the ultimate constituent elements. This may not be the case in the future, and at that time students of behavior will still accept the then current views of physics. Unlike the psychologist, and indeed unlike even the physicist, who carries with him a philosophy derived from church, school, and home, the behaviorist finds no mind, consciousness, or psychic process in the world. Indeed those other students referred to do not *find* consciousness in the world. They merely *assume* that it is there. As science in its progress has found it unnecessary to assume a teleology, so it is now finding that psychic causality is a mystical concept which does not lend itself to experimental work. "From the standpoint of the writer, behaviorism is the science that studies the origin and development of those bodily movements (response) of the individual which establish his status in the social organization of which he is a member. The three parts of the problem are: (1) the stimulating conditions, (2) the sensory-cerebromotor conditions, (3) the behavior conditions, as measured by both the biophysical and biosocial responses."

Part Two is devoted to an analysis of human behavior. It is here that the author elaborates his conception of biophysical and biosocial responses and indicates the scientific futility of the concept of mentalism. Biophysical stimuli and responses concern such events as light and sound waves, on the one hand, and such processes as receptor, neural, and effector changes, on the other hand. The biosocial stimuli are best illustrated by language stimuli and language responses. "All biosocial responses are biophysical reactions but not all biophysical reactions are biosocial responses. The characteristic of the biosocial response lies in its being a factor determining the social status of the individual." The social factor is not a superphysical one. It is derived rather from the equivalence of stimuli and of responses. For example, two men may light cigars in objectively different ways but in ways that are biosocially equivalent. So also words in two languages are biophysically different but biosocially equivalent [to the extent that they control equivalent behavior]. This conception of biosocial responses, particularly if coupled with a thoroughgoing analysis of equivalence, clarifies many of the problems of behavior without recourse to the precarious concept of meaning which has been so exploited by the psychologists. Some critics have said that, if language activities are essential to thinking, then two men speaking different languages could not think alike. Such an

analysis as that by Weiss, however, indicates that likeness in thinking does not depend upon biophysical likeness but upon biosocial equivalence. To the extent that similar behavior is controlled by the two languages to that extent the men in question do think alike.

In the final part of the book Weiss gives a general discussion of the following topics: language, thinking, desire, purpose, volition, motivation, emotion, and affection. In each topic he seeks to disentangle the general outlines of the observable facts from the confusion in which psychology and popular magic has left them. This can be illustrated from the discussion of desire and purpose. If a man says, "I wish I had a home," why call the phenomenon desire, drive, or wish? ". . . . is it not simpler to include it in such a category as home building activities and then investigate the problem from the standpoint of determining at what age and under what stimulating conditions . . . . the home building activity manifests itself?" "It is only in psychology that we find such statements as: the desire for a home can be gratified by eliminating the desire or substituting other desires. This does not represent a scientific problem."

I know no better way to close this review than to cite with approval the following quotation: "The behaviorist affirms that his science is a study of the material, biological, mechanical, and social antecedents that are at the basis of human achievement, and he wonders whether this does not represent the expectation of even those who have given us the traditional formulation of psychology better than does the traditional definition." Such a statement should, of course, not stand by itself because of its inclusiveness; but, in fundamental outlook, it is sound. My own tendency is to define the field by an enumeration of the three or four major problems upon which anthroponomists are at work. One thus avoids, for the present, the difficulties which arise when characterizations are made in general terms, difficulties however which are not embarrassing for the progress of experimental work.

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